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THE EFFECT OF RAPID CHANGES OF WORK ON THE RATE OF PERFORMANCE

BY J. CROSBY CHAPMAN

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Several papers[1] have been published recently investigating the problem of the amount of work produced in successive periods of short duration. The results prove conclusively that in several of the simple mental functions such as addition, opposites naming, color naming, cancellation, etc., there is distinct evidence of the presence of a so-called "initial spurt." That is during the first two or three half-minute periods, the work produced is sometimes as much as 20 per cent. in excess of that performed during the later periods. This factor of initial spurt is so great that the variation in the amount of work produced in successive periods of short duration seems to be worthy of careful quantitative study. In the present paper a cognate problem is attacked, namely the effect of rapid changes of set on the rate of performance of work.

Whenever the individual turns from one type of mental operation to another, a certain change of "set" or "attitude" is required. In complex operations the number of changes of set necessitated must be very great. Taking two mental operations such as those required in a simple addition and an ordinary cancellation test it is desirable to measure the extent to which speed of work is affected by rapid changes of set. This can be accomplished by comparing the rates of work under the two following conditions: (1) when only one oper-

[1] Poffenberger and Tallman, *Psych. Rev.*, Sep., 1915, p. 371; Chapman, *Jour. Educ. Psych.*, Sep., 1915, p. 419; Chapman and Nolan, *Amer. Jour. Psych.*, April, 1916, p. 256; Thorndike, *Amer. Jour. Psych.*, Oct., 1916, p. 550.

ation is performed for a certain period; (2) when two operations are performed alternately for the same period. It was to make the comparison that this study was undertaken.

The general arrangement of the experiment was such as to test the efficiency of a group of subjects on five occasions. On each of these occasions the work was divided up into six trials, each of three minutes; during each of these three-minute trials the work at one operation or both, as the case might be, was continuous. In the table below the arrangement of the experiment during each of the six three-minute trials is shown; *A* and *C* signify addition and cancellation respectively.

Trial	Half-minute Periods					
	1	2	3	4	5	6
I	*A*	*A*	*A*	*A*	*A*	*A*
II	*C*	*C*	*C*	*C*	*C*	*C*
III	*A*	*C*	*A*	*C*	*A*	*C*
IV	*C*	*A*	*C*	*A*	*C*	*A*
V	*C*	*C*	*C*	*C*	*C*	*C*
VI	*A*	*A*	*A*	*A*	*A*	*A*

Thus during trial 1, addition was performed steadily for three minutes; during trial 2 cancellation; while during trial 3 addition and cancellation were done alternately. The only difference between trials 3 and 4 and the other trials was that in the two former there were two types of work, involving a change of set every 30 seconds, while in the rest of the trials only one operation was involved throughout.

This alteration of work was accomplished by having addition on one side of the test blank and cancellation on the other. At a signal every half minute the subjects reversed the sheets and passed from the one operation to the other. The point reached in the case of addition was marked by putting down the partial result of the column not completed. The Thorndike addition and the Woodworth and Wells' cancellation sheets were employed. In order to eliminate every factor except the change of set, when the subjects did the continuous cancellation and addition, they were made to reverse the sheets every half minute and proceed on the reversed side with the same operation. It may be said that

the reversing of sheets was practised during the preliminary periods, so that it became almost automatic.

The subjects were pupils of the junior class of Glenville High School, Cleveland. The nature and operation of the tests were carefully explained to the pupils, but the object of the experiment was not revealed. Particular care was taken, during the conduct of the test, to see that the sheets were reversed at the time when the signal was given; it was pointed out that any delay, accidental or purposive, might vitiate the experiment.

The basis of scoring was ten for each column added correctly, a score on the same basis being given for part of a column added. The score in the cancellation test was on the usual arbitrary basis: 2 (number cancelled correctly)—2 (number omitted)—3 (number wrongly marked). In the following tables the scores are recorded for the total number of individuals taking the test on the particular day.

TABLE I

Day	Period I						Period II						Period III					
	A	*A*	*A*	*A*	*A*	*A*	*C*	*C*	*C*	*C*	*C*	*C*	*A*	*C*	*A*	*C*	*A*	*C*
1 (18 indivs.)	390	367	367	339	372	322	760	724	652	586	684	660	497	736	405	740	447	752
2 (29 indivs.)	755	667	637	596	673	626	1,444	1,450	1,378	1,458	1,312	1,384	728	1,530	657	1,368	672	1,366
3 (24 indivs.)	459	464	526	531	492	550	1,282	1,290	1,168	1,160	1,218	1,228	647	1,284	503	1,220	567	1,246
4 (24 indivs.)	727	605	552	546	546	542	1,582	1,448	1,296	1,240	1,354	1,250	705	1,424	602	1,278	576	1,340
5 (25 indivs.)	580	598	660	576	596	549	1,554	1,376	1,378	1,360	1,412	1,428	586	1,490	672	1,404	666	1,482

Day	Period IV						Period V						Period VI					
	C	*A*	*C*	*A*	*C*	*A*	*C*	*C*	*C*	*C*	*C*	*C*	*A*	*A*	*A*	*A*	*A*	*A*
1 (18 indivs.)	792	456	746	443	816	469	926	898	806	792	760	778	398	367	330	346	346	343
2 (29 indivs.)	1,580	660	1,404	679	1,502	726	1,628	1,572	1,478	1,472	1,462	1,446	720	626	575	573	609	603
3 (24 indivs.)	1,440	576	1,128	583	1,312	597	1,394	1,352	1,232	1,178	1,294	1,246	553	529	531	474	506	504
4 (24 indivs.)	1,472	647	1,370	624	1,262	655	1,482	1,398	1,318	1,290	1,224	1,278	645	556	563	532	526	515
5 (25 indivs.)	1,536	719	1,402	662	1,418	633	1,664	1,508	1,450	1,370	1,438	1,558	674	619	576	560	608	593

Using these data it is possible to compare the amount of addition performed in the periods when there is a change of set with the amount produced when there is continuous work at the same operation. The same results can be deduced in the cancellation test. It will be noticed that in the treatment of the data, owing to the arrangement of the periods, the practice factor is eliminated. The scores in Table II are

worked out on the percentage basis; 100 being the scores obtained during the periods when there is continuous work in the same operation.

TABLE II

		Average	P. E. of Average
Addition	Continuous work	100	1.3
	Change of set	114	1.5
Cancellation	Continuous work	100	1.6
	Change of set	100.6	1.8

From this table it will be seen that the subjects were 14 per cent. more efficient in adding when they were alternately performing cancellation, than when they were adding continuously. On the percentage basis P.E. of difference = 1.3, that is, the difference is nearly eleven times the unreliability. In the case of cancellation the effect of change of set is negligible, for the same amount of work is produced when the operation is continuous as is obtained when there is change from this operation to that of addition, every half minute.

We are thus faced with having to explain the fact that while a change from addition to cancellation, and back to addition, etc., does not seem to exert any effect on the rate of cancellation when compared with continuous work, yet in a mental operation such as addition it causes an actual increase in efficiency of 14 per cent. What is the difference in the two functions which causes the effects of a change of set to be so different? The factor of increased interest could not account for this differential result. The reason is probably similar to that suggested for the explanation of the initial spurt effect, namely that it is a matter of interference. In the case of continuous cancellation, chiefly a motor test, the previous cancellations do not have to be given time to fade away to any marked degree; little has to be banished from the mind when the subject proceeds to the next cancellation, whereas in addition, chiefly a mental function, when the passage is made from one step to another all previous results, except the one necessary for the immediate calculation, must be forgotten. Thus if we imagine an individual commencing with no figures in the mind at all, it would seem reasonable that the individual would go faster in the first few seconds, than in the later seconds, when the mind has of necessity to be given time for

the previous totals to fade away and obliterate themselves. When the subject passes from cancellation to addition, the subject as far as addition is concerned is better off than when passing from addition to addition; such an explanation is necessary to account for the results obtained.

This hypothesis is strengthened by the evidence obtained in the following experiments. Here the conditions were changed so that instead of having cancellation alternating with addition, a period of complete rest was substituted. The distribution of time is shown below.

Trial	Half-minute Periods								
	1	2	3	4	5	6	7	8	9
I	*A*	*R*	*A*	*R*	*A*	*R*	*A*	*R*	*A*
II	*A*	*A*	*A*	*A*	*A*	*A*	*A*	*A*	*A*
III	*A*	*R*	*A*	*R*	*A*	*R*	*A*	*R*	*A*

A = addition. R = rest.

The procedure of the experiment was similar to that employed in the previous experiments, except that on the reversal of the sheet during trial 1 and trial 3, no adding was done. The subjects were 24 students of an undergraduate class. In this experiment the average amount of work done during trials 1 and 3 (periods 3, 5, 7, 9) is compared with the amount done during trial 2 (periods 3, 5, 7, 9). These results are shown in Table III.

TABLE III

Trial	Half-minute Periods			
	3	5	7	9
I	31.3	28.3	32.1	33.1
II	25.8	26.8	26.3	26.3
III	35.1	31.5	31.1	30.1

The comparison of the amount of work produced under (1) conditions of change every half minute from rest to addition; (2) continuous work; is shown in the next table reduced to the basis of 100 for the continuous work.

TABLE IV

		Average	P. E. of Average
Addition	Continuous	100	.3
	Broken by half-minutes rest periods	121	1.6
	Unreliability of difference	1.6	

Here it will be seen that the complete rest alternating with addition, has a distinctly greater effect on the amount of addition performed than the change from cancellation to addition affords. This would agree with the explanation that the decrease in product produced after the first half minute is caused by an interference produced by previous work, for in this case complete rest should have a greater effect than the mere change which is afforded by passing from cancellation to addition.

The results of this paper give strong support to a previous hypothesis,[1] that the phenomenon of initial spurt is probably due to an interference effect which results from continued work. Thorndike[2] also gives this as an explanation of his own results which indicate a higher efficiency at the commencement of the work than during its steady progress. If these contentions are true, we shall have to avoid the phrase initial spurt, which seems to imply, not merely a power of producing a greater external product, but also a greater internal initial effort, and replace it by a term which contains the idea of initial maximum performance produced by lack of interference.

To summarize:—This experiment indicates that rapid changes of set, every half minute, from cancellation to addition, result in a greater combined speed of work than is found when either of the operations occur separately. Far from the change of set involving, as might be expected, a certain time lost in adjustment, it effects an increase of speed. The explanation is probably found in interference. This suggests that the last named factor occupies a more important place than is supposed in mental operations, and that its effects should be measured over much shorter periods of time.

I wish to express my indebtedness to Miss Mansfield who conducted and scored a portion of the experiment, and to the authorities of the Glenville High School for the facilities which were afforded.

[1] Chapman and Nolan, *Amer. Jour. Psych.*, April, 1916, p. 260.

[2] Thorndike, *Amer. Jour. Psych.*, Oct., 1916, p. 565.

APPARATUS FOR RECORDING CONTINUOUS DISCRIMINATION REACTIONS

BY H. C. McCOMAS

Princeton University

A series of discrimination reactions has been used in connection with a number of investigations having widely divergent interests. The records obtainable in time and accuracy for such reactions afford side lights upon a variety of processes. It is not surprising, therefore, to find some type of discrimination apparatus, from Seashore's "Psychergograph" of fifteen years ago to the numerous forms of today, employed in experiments on mental efficiency, concentration of attention, influence of distractions, motor impulses, etc.

The devices used are adapted, of course, to the immediate purposes of each experimenter, though the general principle is the same; namely, to present several different stimuli and to record the several possible reactions continuously. To obtain reliable results in this work a number of conditions must be squarely met. That part of the apparatus with which the subject has to do must be noiseless, with no moving parts that afford distractions. The stimuli must be constant in intensity and sufficiently varied in the order of presentation to obviate anticipation. The type of response must admit sufficient variation to preclude the quick formation of habit. Fatigue in sense-organ and reacting muscles must be reduced to a minimum. Time records should be obtainable for both right and wrong reactions during variable periods.

In addition to these indispensable requirements it is desirable to have an apparatus to which all subjects become quickly adapted. This shortens the practice curves. If the reaction is simple and the discrimination is not complicated, the training period is reduced; and the results of different subjects are more comparable. For a large class of subjects it is a decided advantage to have work which is interesting. The kymograph

records are long and should be easily legible. This can be partially accomplished by markers with wide amplitude and set close together. As this type of apparatus is not manufactured it is desirable that it should be easily and cheaply made.

The apparatus to be described meets these requirements more fully than the usual types. It is capable of many adaptations. The work in which it has been most used was in obtaining ten minute records of continuous discriminations. The procedure was briefly this: the subject sat in one room, with the exposure screen and reacting keys; the experimenter and the rest of the apparatus were in another room. The subject reacted to four differently colored lights, by pressing different keys. He learned which keys extinguished which lights before the records began, and then he reacted as quickly as he could with the fewest possible false reactions. The instant one colored light was extinguished another appeared; the order of their appearance was miscellaneous. The subject therefore controlled the rate of the exposures. The records were read for ten second intervals and showed that the average subject makes from eight to thirteen correct reactions in such an interval and from no false reactions to three or four. The time of reaction and the number of false reactions increase as the test goes on. The records show typical differences in subjects and characteristic forms of reaction for some subjects at different times of the day.

In describing the apparatus it may be conveniently considered in three parts: first, the stimulus and reaction table; second, the automatic switchboard; third, the recording instruments.

Upon a small table is placed a green cardboard screen with a small ground glass window in its center. In a rack behind the window are four Mazda, six-volt bulbs. In front of these are four different colored gelatine strips. The bulbs are connected with a Hyray battery. This service brings the filaments to incandescence in about .05 sec. after a circuit is closed. Four telegraph sending keys are placed in front of the screen upon the table, and in such a position that they

are easily seen in indirect vision when the eyes are fixated upon the window (Plate I.). They are wired to make two circuits; one by the contacts at the far end of the rocking arm and the other by the contacts at the near end (Plate IV., *LC* and *MC*). The reaction movement breaks the circuit in which the light is made and at the same time it makes a circuit which actuates a marker on the kymograph. Thus

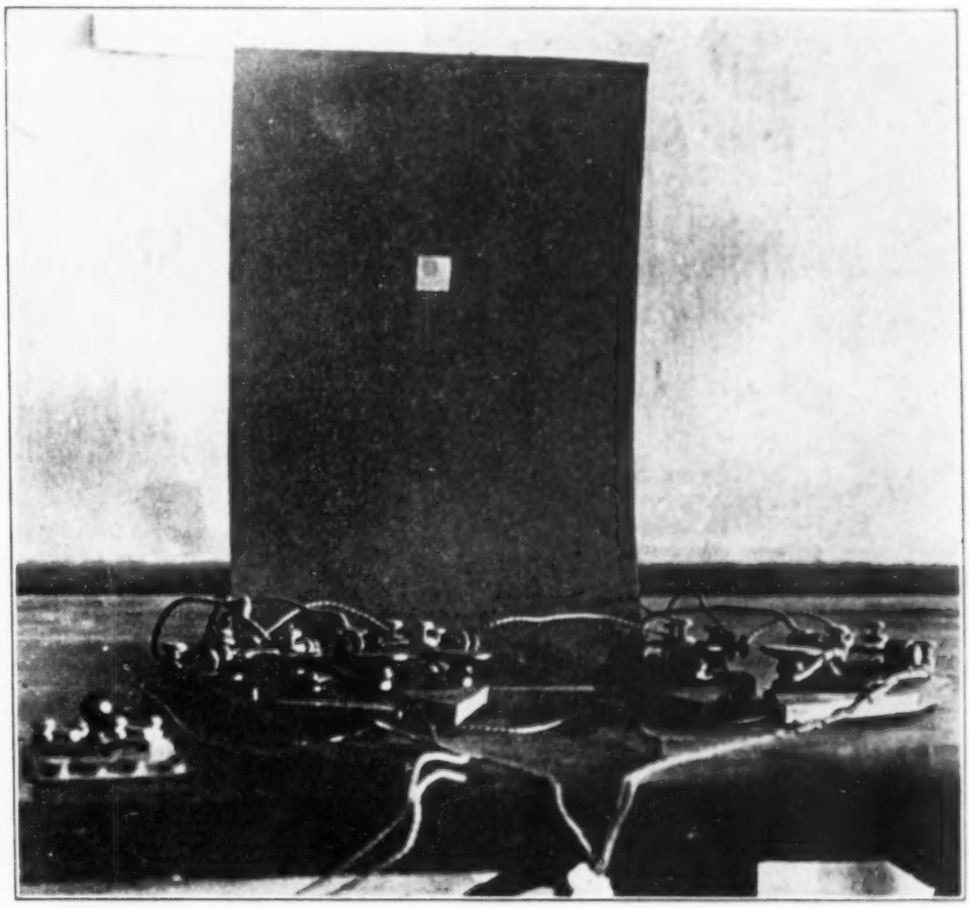

PLATE I.

the light is extinguished and a reaction recorded by one movement.

An automatic switchboard is used to remake the light circuits. This device is easily and cheaply made by fastening a 'secondary' electric clock opposite a board on which there is a circle of radiating copper contacts (Plate II.). The 'secondary' is a simple mechanism by which the minute

hand of the clock is moved forward with each break of the current running into the magnet. By placing a trailer contact on the minute hand and adjusting it to pass over the radiating copper contacts, it is possible to have the hand move forward to a new contact and make a new circuit whenever a circuit is broken by a reaction. The sixty copper contacts are fastened upon the supporting board as radii coincident with the minute divisions of a clock face. From the contacts wires pass through the board and rest in four bowls of mercury, fifteen wires to the bowl. From each bowl a wire runs to

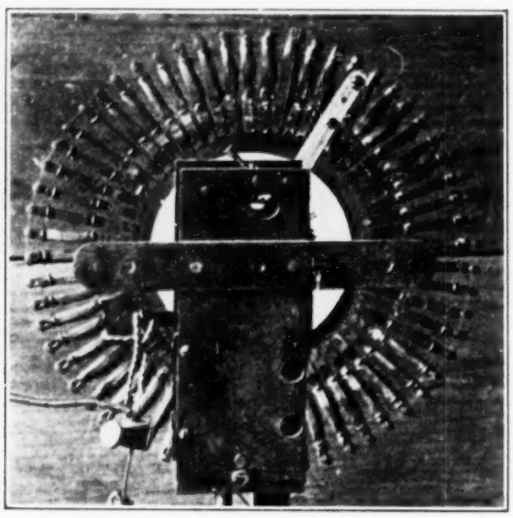

PLATE II.

each of the four keys. When a light circuit is broken at one of the reaction keys, such as *LC* in key K_4, Plate IV., the light L_4 is extinguished, the current ceases to pass through the coils in magnet *M*, this releases armature *A* and the arm *AR* swings counter clockwise moving the hand *H* forward to another contact. The time required to pass from one contact to the next is .08 sec. This can be shortened by placing the copper strips closer together, and by adding to the weight at *N*.

The records are taken upon a kymograph. A Jaquet marker gives the time in seconds. A marker in circuit with the armature of the clock registers each correct reaction and a marker in circuit with the reacting keys scores all the reactions

made. These markers are constructed from two telegraph sounders with long pliable copper strips to give a wide amplitude (Plate III.). They are set close together, the all-reactions registering immediately below the correct reactions.

The wiring of these three divisions of the apparatus can be easily understood by reference to Plate IV. There are two independent battery circuits, the light circuit and the marker circuits. The light circuit may be traced from the battery B_1 to the four bulbs $L_1L_2L_3L_4$ and from these to the four reacting keys $K_1K_2K_3K_4$. If all the circuits stood open except K_4, the current would pass through LC into the wire R_4 running to the automatic switchboard. There it would enter

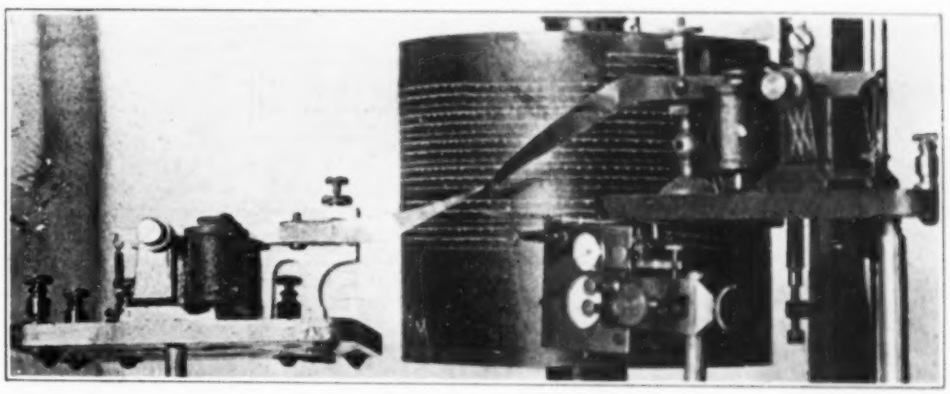

PLATE III.

one of the four mercury bowls from which it would pass to the radiating copper strips CS. On one of these the trailer contact H is resting. The current, therefore, passes through H down to the magnet M, actuating armature A and thence back to the battery.

The other two circuits lead from the battery B_2, they are the marker circuits. With the reaction to each light the current in M is broken and the lever AR moves about the axle P. With each movement a contact is made at N and the marker Y registers on the kymograph KY giving a record of the right reactions. The marker Z is actuated by closing such a contact as MC in K_4. The wires F_1 and F_2 connect the keys with the battery and the marker.

If it is desired to give a longer series of discrimination

reactions than sixty at a sitting it may be advisable to put a sliding switch in $R_1R_2R_3R_4$ and thus shift the order in which the lights appear in $L_1L_2L_3L_4$. However, many subjects do

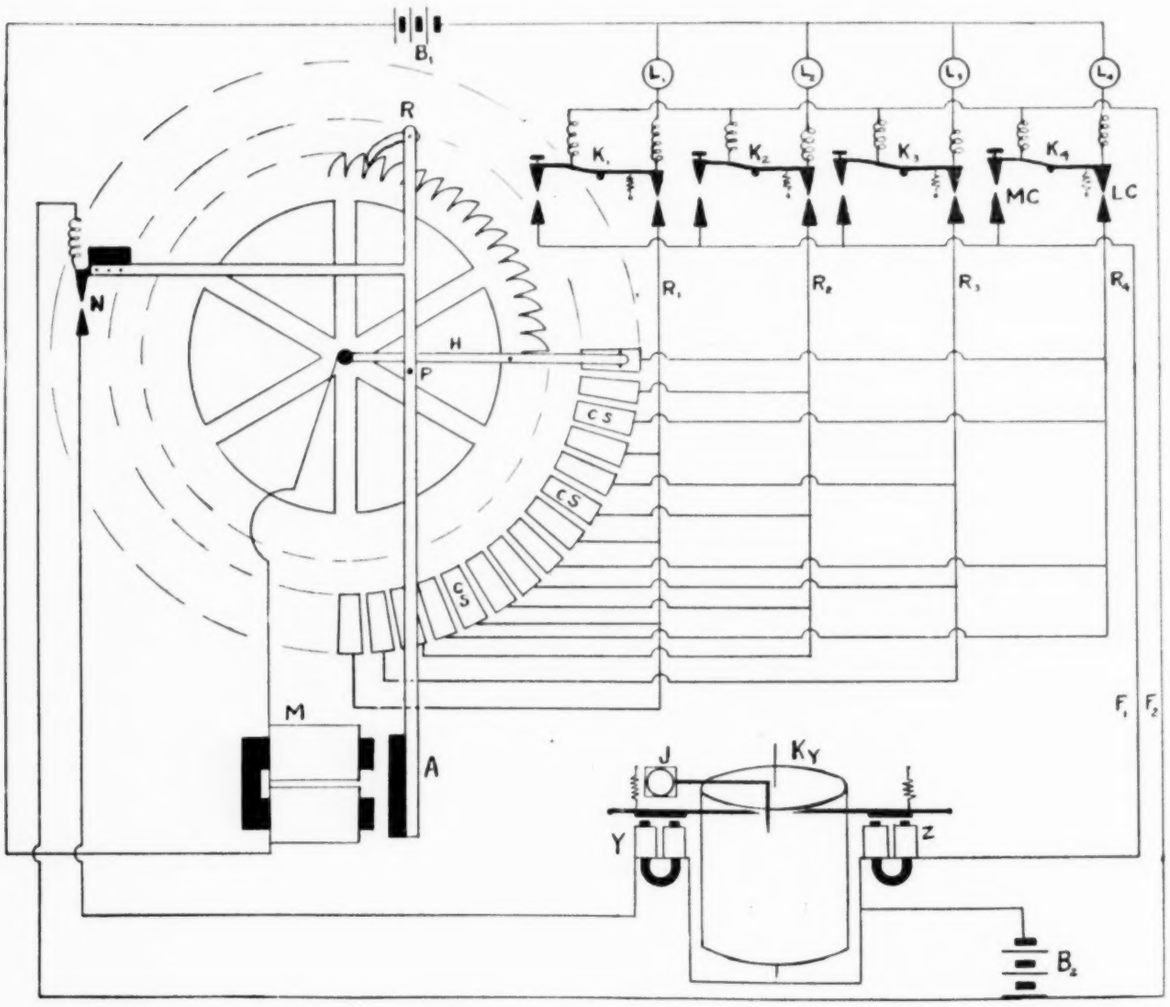

PLATE IV.

$L_1L_2L_3L_4$, lamps; $K_1K_2K_3K_4$, reaction keys; *LC*, contacts for lighting circuit; *MC*, contacts for marker circuit; $R_1R_2R_3R_4$, connections with automatic switchboard; F_1F_2, connections with marker; *CS*, copper strips on clock; *H*, clock hand connection; *M*, magnet; *A*, armature; *R*, ratchet engaging wheel; *P*, axle suspending arm, *R–A*; *N*, contacts; *KY*, kymograph; *J*, Jaquet time marker; *Y*, marker of right reactions; *Z*, marker of all reactions; B_1B_2, batteries.

not learn the order or any part of it, though it is repeated ten times in ten minutes.

The above arrangement of exposure and recording apparatus gives a very serviceable device for discrimination work. There

are no distractions. The eye may remain fixed on the stimuli. The reactions are simple and not fatiguing. The order of presentation is not repeated for sixty exposures and the right and wrong reactions are both recorded. Not the least attractive feature is the interest the reactions arouse in the subject, who willingly struggles with a long series "for the fun of it."

EXPERIMENTS IN BALL-TOSSING: THE SIGNIFICANCE OF LEARNING CURVES

BY JOSEPH PETERSON

University of Minnesota

Swift's curves of learning to keep "two balls going with one hand, catching and throwing one while the other is in the air," do not show the rapid initial decline characteristic of most motor learning curves. On the contrary they are in general "concave toward the vertical axis, which means, of course, that the progress was at first slow and then more rapid."[1] In Swift's curves vertical distances represent the average number of successful catches in ten trials, a day's practice; and the horizontal distances indicate the successive days, or the number of practice periods. By this method, as Thorndike has pointed out,[2] equal distances on the abscissa line do not mean equal amounts of practice. A rearrangement of the data of one of Swift's subjects so that the horizontal distances will stand for equal number of tosses results in a curve which up to 4,500 tosses may be roughly represented by a straight line.[3]

In animal and other experiments in which errors are significant, as is true also of ball-tossing, curves of errors have been given along, usually, with curves of other data. Just what data to take as the most significant in any learning process, is a question that is not settled, and one that must obviously be answered differently for different learning processes. In most cases, even when time, errors, and performance are plotted in separate curves, the data are liable to be misleading if taken to represent the entire learning that goes on. If, for instance, one of the curves shows a plateau

[1] Swift, E. J., 'Mind in the Making,' 1909, p. 177. The interpretation expressed in this quotation is erroneous, though it is very frequently expressed in psychological literature.

[2] Thorndike, E. L., 'The Psychology of Learning,' 1913, p. 121.

[3] Thorndike, *op. cit.*, p. 122.

we are too apt to conclude that learning was abated or wholly suspended at this point. This tendency is especially noticeable in cases of learning represented by a single curve.

The Experiment

The experiment here reported was devised to secure extensive data on ball-tossing in terms of errors, so that the results might be compared with those of other kinds of experimentation. Though other data—time and 'the feeling of efficiency' before practice—were obtained, the conditions of the experiment were not such as to make these data very reliable. They will therefore be considered only in a general way in the discussion.[1] The subjects in the experiments were sophomore, junior, and senior students taking their second half year's course in psychology. The experiment was performed in the spring of 1916. Each subject received written instructions as follows:

"Get two solid rubber balls, each of about 42 mm. diameter and weighing approximately 123 grams. Select for practice each day a period as free from interruption as possible, preferably in the morning before breakfast (or two periods daily, one in the morning and one in the afternoon). The place of practice, as well as the time, should be constant. A room with high ceiling and light walls is best; varying conditions of the sky would interfere with outside practice. Arrange something to catch, and thus to prevent from rolling away, the balls you miss. Practice the act of keeping two balls going with one hand, catching and throwing the one while the other is in the air. (This was illustrated to all subjects by the writer.) Count only the catches, beginning with that of the first ball tossed and ending with that preceding the first miss. This is one series. Each practice period is to continue until the total number of successful catches reaches 200, when it must stop. The two-hundredth, if stopped voluntarily, is not a miss. Rest between series whenever tired, but only for one minute. Continue, practicing regularly every day, until for five days you have made no misses.

"Keep a careful record of the number of catches in each series, the number and place in the practice period of rests, cause of miscatches when easily noticeable, time of day, conditions of light (never artificial), your own condition (and whether you feel that you are going to make a good record), chief difficulties and how overcome, total time of each practice period, and any other observations. Follow the directions closely, noting any departure from them. Be constantly on the alert for factors or conditions enabling you to explain any irregularities."

The instructions are slightly changed as published here, but only in unessentials, such as the rearrangement of sen-

[1] In general it may be said here that time curves seem to parallel rather closely error curves and that one's 'feeling of efficiency' before practice is, on the whole, not a very reliable index of efficiency as measured by actual results.

tences, and the making more explicit of detailed matters explained orally to all subjects together. All who found it possible were urged to take two periods of practice daily, of 200 catches each, so that we might have data on the distribution of practice. In such cases one practice was to take place in the morning and one in the afternoon, never both in the same half day. There were, of course, slight deviations in the sizes of the balls used by different individuals and slight irregularities in other details of practice. Occasionally, for instance, a day's practice would be omitted because of illness or absence, some of the students were obliged to practice throughout the experiment in artificial light, and occasionally one would forget in one's enthusiasm and run over the 200 limit of catches. All papers showing irregularities or carelessness judged sufficient to interfere with the reliability of results were excluded before the data for the writing of this report were collected. That is to say, the papers used in the report were *selected* only on the basis of regularity and carefulness of work, not on that of the kind of results secured! In all individual records used the details of the experimental procedure and results were sufficient, in connection with individual conferences and reports during and after the experiments, to enable the writer, the director of the experiments, to judge of their reliability. No records are included of individuals who had had previous practice in the performances, even though the subject had apparently lost the effects of the practice.

In all, the records of twenty-six individuals are included in the reports, though the data from three of these records are excluded from the averaged curves because the practice in each case was discontinued before a high degree of skill was acquired. Two of these were taken from the once-a-day group and one from the twice-a-day group. For reasons already explained the records of eight subjects were excluded from any consideration in this paper. The total number of persons performing the experiment was therefore thirty-four.[1]

[1] It should be stated also that the experimenter himself practiced at ball-tossing daily for three months in the spring of 1914. In the following year he obtained results

Results

The individual results of these experiments are given in the following tables and figures, so that the reader may see the extent of individual variations and that he may judge for himself on important points mentioned in the following discussion. It is important, moreover, to preserve these individual records, which in each case represent probably from ten to fifty hours' experimentation, so that they may be available for other investigators who may be doing work of a similar kind. Mere averages, we must also keep in mind, not infrequently cover up significant aspects of individual results, such as plateaus, initial 'spurts' and changes near the physiological limits of learning. In results such as these, deviations from central tendencies are not particularly illuminating, and have therefore not been given. They may be calculated by any one who desires them. It is more important for our purpose to give the individual results and the averages.

Explanatory Note.—In Table I. and Table II. a horizontal line between any two numbers in a column indicates one practice period missed; if more periods than one were missed the exact number is indicated by an italic figure at the right of the line. The columns bracketed at the beginning and end are not included in the averages given in Table III.

Table I. gives the results of the fifteen subjects who had one practice period each day. Table II. is the corresponding record of the group of thirteen subjects who had a practice period in the morning and one in the afternoon, or two in twenty-four hours. The first column, P. P., gives the number of the practice period. For example, 20 in the P. P. column in Table I. is the twentieth day of the practice and also the twentieth practice period, whereas in Table II. it represents the afternoon practice of the tenth day. The other columns give the number of errors—miscatches—made by the respective individuals in the practice periods of 200

from fifteen subjects, which agree substantially with those here reported. These results are not incorporated in the data of this report for the reason that the same uniformity of conditions of experimentation was not secured. The method used in the present grew out of the earlier work.

TABLE I

INDIVIDUAL RECORDS OF ONCE-A-DAY GROUP. ERRORS IN 200 CATCHES

P. P.	Subjects														
	A	*B*	*C*	*D*	*E*	*F*	*G*	*H*	*I*	*J*	*K*	*L*	*M*	*N*	*O*
1	209	104	190	111	79	155	157	120	153	64	38	72	122	105	116
		—2													
2	195	55	185	42	53	128	166	69	113	37	42	25	102	68	78
3	181	45	187	44	34	107	168	72	120	42	19	21	100	63	65
4	165	40	175	42	27	76	150	52	83	19	23	23	70	60	55
5	152	37	172	42	24	56	145	62	88	22	13	10	86	49	42
6	143	36	169	21	13	49	147	53	81	16	12	11	75	49	35
7	154	34	158	19	11	37	139	48	89	15	9	11	65	37	22
8	129	28	155	30	10	35	132	45	85	12	10	6	62	32	26
									—2						
9	116	26	143	26	10	30	126	44	75	13	11	8	63	26	28
		—4													—3
10	119	14	141	24	9	26	111	44	78	11	15	7	57	19	16
															—2
11	118	13	145	18	8	20	103	45	80	15	12	2	53	14	18
12	107	8	138	16	10	19	93	45	81	11	6	12	39	12	15
13	109	9	139	17	4	26	135	42	77	9	13	4	40	11	14
14	105	8	135	15	9	20	77	37	77	12	11	3	37	11	14
		—4													
15	112	9	121	16	8	20	71	34	77	10	12	2	38	9	17
										—7	—2				
16	103	5	112	9	8	15	55	36	80	11	5	6	44	10	15
		—													
17	108	12	110	9	6	14	55	36	70	10	4	8	34	8	13
		—6													
18	121	6	102	9	4	23	57	36	69	7	7	3	28	8	16
								—	—						
19	117	10	96	8	9	16	55	35	65	5	5	2	29	6	12
20	107	10	96	7	9	14	54	37	79	7	3	3	20	7	7
21	103	10	96	6	5	15	55	37	68	7	4	3	27	7	6
							—								
22	104	10	85	6	5	12	53	32	69	5	6	3	20	6	5
									—2		—				
23	106	6	79	9	5	11	53	25	66	6	4	1	26	5	5
24	109	9	65	9	5	11	50	24	63	5	3	3	33	4	6
		—				—									
25	103	10	63	10	6	20	55	24	64	5	2	5	21	4	5
26	101	7	67	7	8	18	51	27	66	5	4	3	23	4	4
27	106	10	57	6	4	14	50	26	53	4	3	2	18	4	3
		—													
28	109	8	59	7	4	15	45	24	58	4	3	3	14	4	8
									—						
29	102	8	45	9	5	13	77	24	45	4	2	3	14	4	4
		—6													
30	100	5	43	2	4	13	55	23	46	4	2	10	18	4	5
														—	
31	105	5	*	8	5	10	47	22	44	3	0	5	13	11	3
32	108	5		14	6	9	39	23	40	3	0	4	11	11	5
		—													
33	104	5		7	5	19	46	18	36	3	2	3	12	8	2
		—2													
34	99	2		9	5	16	35	15	44	4	2	2	11	7	3
		—2				—					—2				

P. P.	Subjects														
	A	*B*	*C*	*D*	*E*	*F*	*G*	*H*	*I*	*J*	*K*	*L*	*M*	*N*	*O*
35	107	7		3	6	10	36	18	33	4	2	3	8	7	2
36	102	2		4	4	9	33	12	34	2	0	4	7	7	3
37	106	3		7	4	9	31	13	31	2	0	3	8	7	0
38	97	1		5	4	9	30	14	37	3	0	3	16	6	1
								—3							
39	99	*		7	5	8	27	13	28	2	0	4	7	6	1
40	101			7	7	8	33	12	28	2	0	2	12	6	1
41	100			5	4	8	30	12	32	2	*	3	6	5	1
						—									
42	104			5	5	10	30	7	29	3		4	10	5	0
43	102			6	3	9	39	8	26	2		3	8	4	1
44	*			7	5	9	34	4	28	2		5	12	3	1
45				3	4	8	29	8	24	2		2	8	3	0
46				5	4	10	24	5	25	2		4	7	3	2
47				5	2	10	23	6	24	2		10	9	3	2
48				9	2	9	22	6	22	2		5	7	3	1
												—			
49				5	3	7	27	7	20	2		2	8	2	1
50				4	4	5	35	*	18	2		2	5	2	0
51				4	2	*	30		24	2		2	8	2	0
52				4	3		26		17	2		3	5	2	0
53				5	3		21		20	0		3	11	2	*
54				2	0		18		18	0		0	6	2	
55				2	1		12		*	2		2	9	2	
										—5					
56				3	1		9			2		1	4	1	
57				3	1		11			0		2	4	1	
58				1	1		7			0		1	5	1	
59				1	1		4			0		2	8	1	
60				3	2		3			0		4	4	*	
61				1	2		*			0		4	3		
62				4	2					*		1	4		
63				1	3							2	5		
64				1	1							2	6		
65				3	*							2	4		

(*D*'s record for successive days continues as follows: 3, 2, 2, 0, 1, 1, 0, *; *L*'s 0, 2, 2, 2, 2, 2, 1, *; *M*'s 5, 4, 3, 6, 6, 5, 4, 8, 4, 2, 3, 0, 4, 5, *.)

successful catches each. Horizontal lines between any two numbers in a given column indicate an interruption at that point in the practice, due to one of several conditions, such as temporary illness, absence from the place of practice, etc. A small number at the right of one of these lines shows the number of periods omitted. When no number is given with the line only one period was missed. Thus the record of '*B*' shows two periods missed between the ninth and the tenth, one period between the sixteenth and the seventeenth, and so on. This student has the poorest record so far as regu-

TABLE II

INDIVIDUAL RECORDS OF TWICE-A-DAY GROUP. ERRORS IN 200 CATCHES

P. P.	Subjects												
	P	*Q*	*R*	*S*	*T*	*U*	*V*	*W*	*X*	*Y*	*Z*	*AA*	*BB*
1	103	161	98	47	91	148	200	66	72	117	87	174	213
2	79	143	95	34	74	113	178	52	62	58	80	146	205
3	66	112	93	20	69	110	137	46	26	57	38	109	208
4	62	111	91	9	46	111	121	38	25	76	27	96	200
5	52	78	90	7	33	97	123	33	17	24	30	70	203
6	48	68	87	7	26	79	121	55	16	23	29	61	200
7	48	66	85	4	18	86	109	28	19	19	15	44	195
8	37	55	84	5	26	108	109	21	12	13	17	40	200
9	34	57	83	5	17	79	121	18	9	12	15	37	180
10	34	59	80	2	17	88	91	17	13	10	18	33	187
11	28	36	78	4	17	65	102	15	8	8	12	28	185
12	31	29	75	3	12	73	77	13	8	10	13	28	165
13	37	29	74	3	13	71	102	12	9	9	9	27	163
14	34	42	73	3	11	53	71	12	6	6	11	25	170
15	32	30	71	1	10	50	109	9	6	5	11	26	173
16	26	32	69	1	12	40	82	8	8	6	11	27	168
17	27	22	65	3	11	42	81	9	5	5	12	27	150
18	31	28	60	1	7	39	71	7	10	4	11	30	153
19	20	23	58	1	12	27	74	6	6	4	10	41	149
20	22	26	55	2	12	25	80	9	8	8	9	32	154
21	22	21	51	1	8	33	69	7	4	4	8	30	152
22	21	25	49	1	11	35	65	6	6	4	8	25	135
23	16	18	48	1	5	27	48	5	6	4	5	24	151
24	25	15	43	1	6	25	44	5	11	7	6	20	140
25	21	21	40	1	6	33	36	3	6	3	4	20	147
26	14	18	37	1	6	29	36	3	7	6	4	19	138
27	14	20	30	0	6	25	50	2	3	3	5	20	120
28	13	15	26	0	7	28	64	2	4	6	5	20	127
29	14	15	25	0	4	22	53	2	3	2	5	18	100
30	16	18	21	0	7	22	50	1	3	4	4	15	105
31	9	12	35	0	8	23	55	1	3	3	5	17	109
32	10	11	29	*	7	21	40	3	3	3	5	16	105
33	8	17	24		7	24	54	1	4	3	2	18	95
34	8	18	21		4	22	37	2	3	2	5	21	92
35	10	18	19		7	21	41	2	2	5	4	12	99
36	5	18	17		3	23	63	2	3	3	7	9	85
37	15	13	15		6	21	46	2	4	3	5	8	94
38	7	15	19		1	25	33	3	2	3	6	7	110
39	10	13	16		4	27	37	3	2	1	2	9	80
40	3	12	14		4	20	37	2	2	2	2	16	84

P. P.	Subjects												
	P	*Q*	*R*	*S*	*T*	*U*	*V*	*W*	*X*	*Y*	*Z*	*AA*	*BB*
41	7	12	10		5	14	53	1	2	2	0	17	70
42	5	12	9		3	19	39	1	1	2	4	9	72
43	4	13	12		5	21	41	1	2	1	3	8	76
44	3	11	23		5	17	42	1	0	2	1	5	74
45	4	12	20		6	13	31	1	3	1	0	5	78
46	4	10	17		2	14	33	1	1	1	1	6	65
47	8	9	14		3	8	37	1	1	1	0	10	72
			—2										
48	3	11	13		3	10	34	3	1	2	0	4	74
49	6	12	11		5	10	27	1	1	2	2	4	65
50	6	14	9		6	13	38	3	0	1	1	6	60
51	2	10	9		5	14	33	1	0	2	3	4	67
52	3	10	8		4	10	30	1	0	1	0	3	69
53	3	7	12		4	12	32	2	1	2	0	2	65
54	5	4	10		1	12	28	1	0	1	2	3	74
55	5	9	9		4	10	35	1	0	2	0	5	72
56	4	6	8		2	7	31	2	0	1	1	3	55
57	3	9	7		0	5	35	1	0	1	1	2	55
58	2	7	7		4	10	34	0	0	1	0	0	57
59	6	5	8		2	8	21	1	*	1	0	3	65
60	3	8	7		2	8	25	1		1	0	2	75
61	5	9	7		2	12	31	1		1	0	0	87
62	4	4	7		4	12	15	0		1	0	0	70
63	4	6	6		3	11	23	0		1	*	2	72
64	3	6	6		4	5	35	0		0		*	66
65	3	5	6		3	7	23	1		0			60
66	3	4	6		4	7	25	1		0			71
67	5	4	7		2	2	24	2		0			57
68	2	2	6		1	6	19	0		0			55
69	5	5	7		5	9	24	0		*			64
70	3	1	6		3	8	28	1					67
71	4	3	4		2	12	16	*					62
													—2
72	3	5	5		0	9	22						62
													—2
73	4	2	5		1	9	15						72
74	2	5	5		3	5	16						57
75	2	5	5		2	11	19						65
													—2
76	3	5	6		2	6	23						55
													—2
77	2	2	6		0	8	21						68
78	5	4	4		0	6	18						65
							—5						
79	5	4	4		0	12	16						70

P. P.	Subjects												
	P	*Q*	*R*	*S*	*T*	*U*	*V*	*W*	*X*	*Y*	*Z*	*AA*	*BB*
80	2	2	4		o	8	16						64
81	5	6	5		o	13	16						60
82	2	7	4		*	4	23						62
83	5	3	4			4	30						58
		—3	—										
84	5	4	3			1	15						*
	—2		—										
85	4	3	4			5	15						
86	3	4	3			8	10						
							—3						
87	3	5	3			3	27						
88	1	5	4			6	12						
		—											
89	1	1	4			4	24						
	—												
90	4	7	4			3	17						
91	2	6	3			4	13						
		—	—			—	—						
92	5	4	3			3	20						
93	3	3	3			5	20						
94	3	4	3			4	12						
						—							
95	1	3	2			5	10						
96	1	1	3			5	9						
97	2	3	2			6	11						
98	2	1	2			7	10						
99	1	2	*			1	15						
	—												
100	3	0				6	11						

(The four records incomplete at this point go as follows: *P*, 1, 0, 1, 0, 1, 0, 0, 0, 0, 0, *; *Q*, 0, 0, 5, 1, 3, 2, 2, 0, 0, 0, 6, 2, 3, 1, 2, 0, 3, 1, 2, 0, 1, 0, 0, 1, 0, 0, 0, 0, 0, 0, *; *U*, 3, 3, 4, 6, 2, 3, 2, 2, 3, 2, 3, 4, 6, 2, 7, 3, 4, 3, 4, 6, 6, 3, 4, 4, 3, 4, 4, 1, 3, 3, 2, 1, 1, 1, *; and *V*, 13, 15, 8, 8, 9, 10, 13, 4, 8, 15, 5, 10, 8, 11, 5, 7, 6, 13, 5, 6, 6, 8, 6, 7, 3, 6, 7, 6, 6, 11, 4, 12, 5, 4, *. Two of these, it will be noted, continue to the 134th period of practice, and are discontinued still unlearned, or below the standard set.)

larity of practice is concerned. The results of these irregularities are not obvious, though they no doubt affect the validity of any conclusions that may be drawn from the averages of the two groups as to the relative values of one practice a day and two practices a day.

Figs. 1 and 2 show the individual learning curves. Each curve is to be identified by the letter corresponding to the column in the tables which it represents. In these curves vertical distances represent the number of errors, or miscatches, and the horizontal distances are successive practice periods of two hundred catches each. It is important to

note the frequent absence of plateaus in those curves which are carried to, or nearly to, a point of zero errors in 200 catches. Too much must not be made of this fact, however, as we shall see when we come to the interpretation of these, and of other, learning curves. The curves are marked by rapid initial declines, having the general form of the so-called normal curve of motor learning. *Up to the present time this rapid initial decline in errors or in time has been interpreted as showing a corresponding rapid initial learning—an unfortunate error*, due largely, it appears, to the ease with which 'errors' and time in seconds are measured, and to the lack of thoroughgoing criticisms of the significance of any such arbitrary measurements of learning. Various ingenious reasons why there should be rapid initial learning corresponding to the rapid initial decline of the error, or time, curves have been 'thought out.' We shall later on consider this matter more fully.

Cross lines on the curves, and the numbers that occasionally occur with them, indicate practice periods omitted, as has already been explained in connection with the tables.

Many interesting individual differences are noticeable in Figs. 1 and 2. *A* was a student of high rank in the class work. Her curve seems to indicate that some peculiar, obstructing habit developed early in the experiment, bringing the curve almost to a plateau. It is impossible to say how the curve would have developed if the experimentation had continued to the point of completion. In such a case a little help by the experimenter at the point of discontinuance would doubtless have resulted in a rapid drop at this point. Without such help *A* seemed to have approached a sort of pseudo-physiological limit, the learning likely being limited or obstructed by the formation of habits for certain constant errors. Curve *I* is a very peculiar one; it is also the curve of a woman. This student was older than most of the others, was above the normal in class standing, but was evidently less adaptable in this kind of physical performance than most of the other students. *C* and *G* are also women making poor ball-tossing records, *C* making also only a passing record

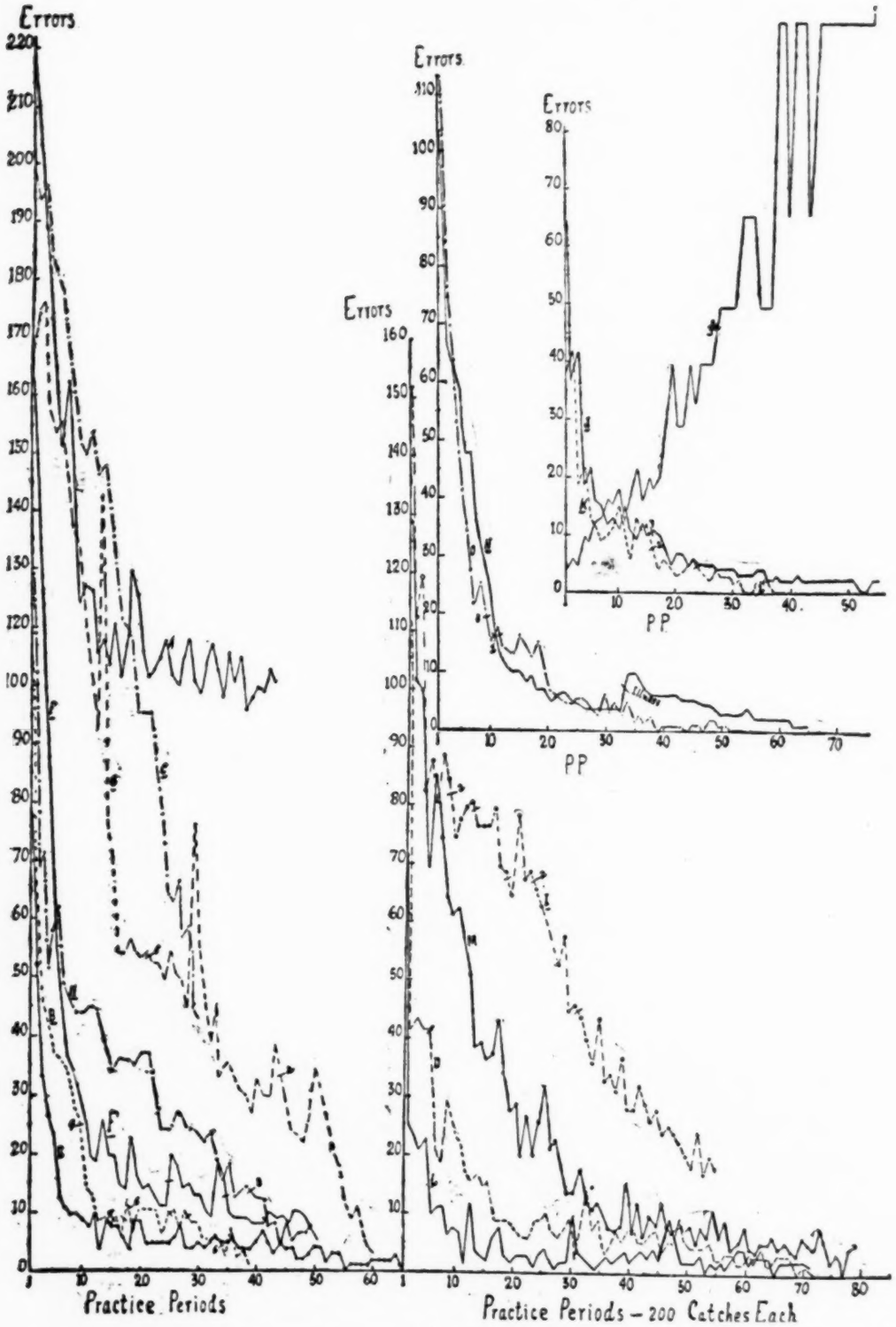

FIG. 1. Individual error curves of fifteen subjects practicing once a day. The small cross-lines and figures indicate practice periods missed. *Ja* is the curve of *J*'s average number of catches per practice period. The learning of any other subject may be given in a similar way. Letters underscored indicate women.

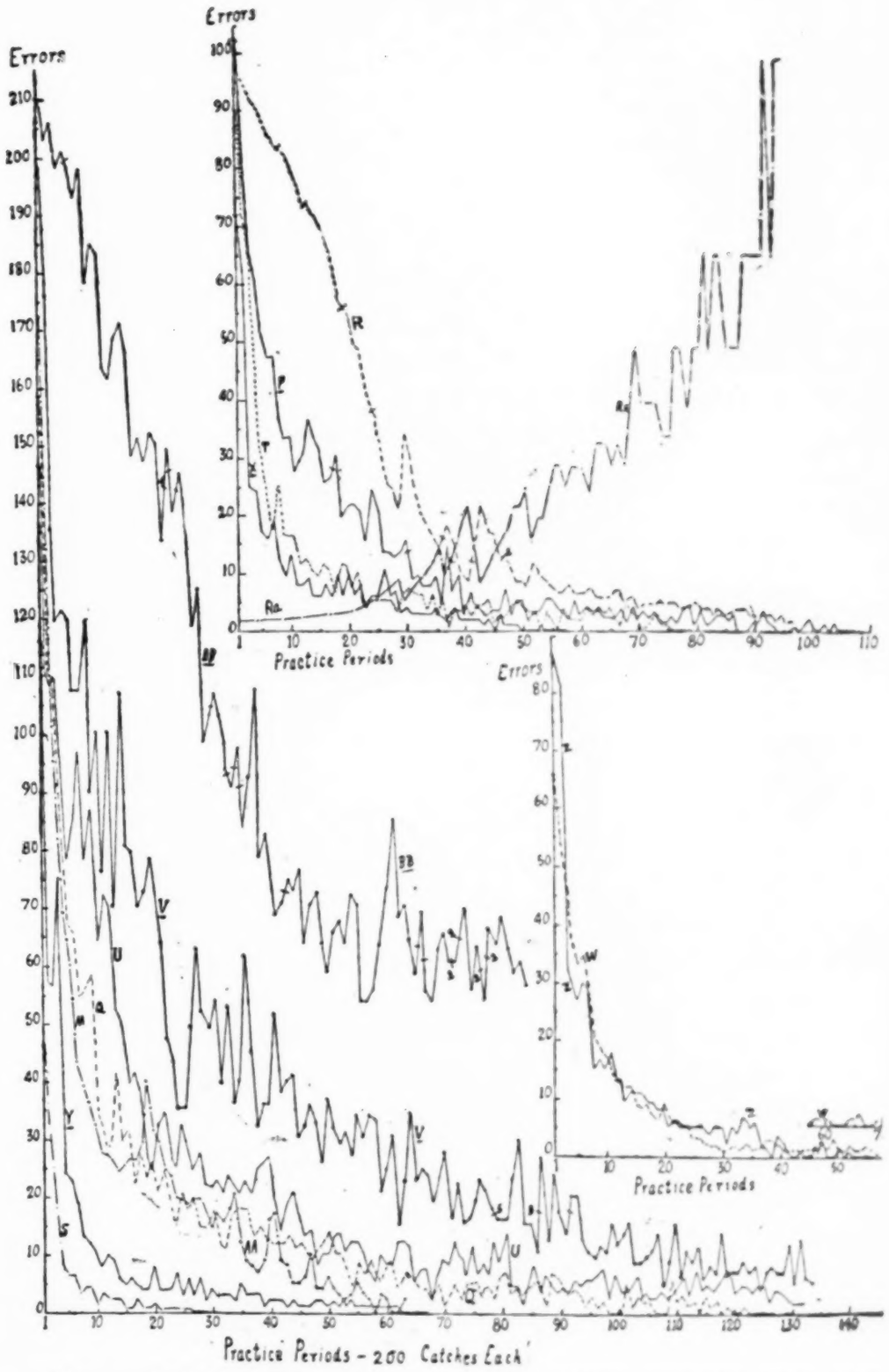

FIG. 2. Individual error curves of thirteen subjects practicing twice daily. Omissions of practice periods are indicated as in Fig. 1. *Ra* represents *R*'s learning in terms of average number of catches per practice period. Letters underscored ndicate women.

in the psychology class. That women do not make the poorest records invariably is evident from the curves of *B*, *E*, *J* and *K*, all women. In Fig. 2 the three students making the poorest progress in the experiment are *V*, *BB*, and *R*, the first two being women and the last a young man who received a D grade in the psychology course. Here again *V* was one of the very best students in two sections of general psychology taught by the writer. *Y* is a woman of C grade in her class, and *S*, making the best record of all in the experiment, is a man who has had considerable experience playing ball. From these records it would be hard to give any reliable information for prediction as to one's success at the performance here practiced. Early interest in ball playing and practice at throwing and catching balls as children do in ball games would seem to be one of the best indicators of success in this experiment, but even here we could not say whether the practice had helped or whether the particular nerve and muscle conformation inherited had predisposed the subject both to interest in ball playing and to ability in ball juggling. The latter would seem to be the more probable condition.

Curves *Ja* and *Ra* should be studied carefully in relation to their reciprocals *J* and *R*.[1] It will be noted that, in conformity with the view suggested in this paper, the error curve changes most rapidly in the early part of the learning where the average attainment is small, or where the series are short. Each series is necessarily ended by an error. In the initial learning, then, there are a great number of series, or different trials, and hence a very small change in each, on the average, makes a big change in the total errors. The error curve is therefore liable to fluctuate more in the initial phases with the average curve advancing more constantly, while in the final part of the learning the error curve will assume a more constant or smooth appearance while the average curve will fluctuate between wide limits. If one to

[1] Note that the term 'reciprocal' is used here not in the strict mathematical sense of values whose product is equal to unity. Their product, with a slight qualification developed in subsequent pages, is equal to a number representing *K*, the constant of practice.

two hundred catches can be made successfully in a series, the total errors for two hundred catches will not change more than one or two units in successive practice periods, while the average number of catches may vary from less than fifty up to two hundred absolute units.

This reciprocal relation obtains between errors and average number of catches because the former directly condition the latter and *vice versa*.[1] This state of affairs is not so obviously present in some types of learning processes, but as we shall see it is well worth enquiring into the relation of errors to attainment in most such experiments.

Table III. gives the averages of the individual errors, column A. E. For reasons already explained these averages do not include the records of subjects *A*, *C*, and *BB*, bracketed at the top and the bottom of the columns. In determining these averages it has been necessary to take note of the fact that some of the columns terminate before reaching zero, no errors. In such cases the averages have been weighted as follows. If, for example, ten columns were being averaged, and one of these discontinued with the 50th practice period (P. P.), it would leave only nine columns from this point. Suppose the last five records of the column terminating here were 8, 5, 6, 6, 7. Suppose also that the sum of the remaining nine records for the fifty-first practice period was 27, then it would seem fair to add to the average, 3, of the nine columns about .3 $\left(i.\,e., \frac{27+6}{10} = 3.3\right)$. The amount added would be gradually decreased to represent the decrease that the discontinued record would probably have had if it had been continued. This weighting of averages is to prevent the appearance in the curve of a sudden change that might be misinterpreted. None of the averages in the twice-a-day group is weighted. Only the last seven averages in the once-a-day group are weighted; these are of course less

[1] An error in ball-tossing ends in fact one *trial* (termed in this paper a series); and it is obvious that the number of trials *times* the average number of catches per trial *equals* the total number of catches. Hence dividing the number of trials ('errors') into the total number of catches we get the average number of catches, 'average attainment.' See next page.

TABLE III

AVERAGES OF ERRORS IN 200 CATCHES AND AVERAGE NUMBER OF SUCCESSIVE CATCHES[1]

Once-a-day Group						Twice-a-day Group					
P. P.	A. E.	A. C.	P. P.	A. E.	A. C.	P. P.	A. E.	A. C.	P. P.	A. E.	A. C.
1	107	1.8	34	12	16.7	1	97	2.1	34	12	16.7
2	75	2.6	35	11	18.2	2	93	2.2	35	12	16.7
3	69	2.9	36	9.3	21.5	3	74	2.7	36	13	15.5
4	55	3.6	37	9.1	21.9	4	68	2.9	37	12	16.7
5	52	3.9	38	10	20.0	5	55	3.6	38	10	20.0
6	46	4.3	39	8.3	24.1	6	50	4.0	39	10	20.0
7	41	4.9	40	9.1	21.5	7	45	4.4	40	10	20.0
8	39	5.1	41	8.3	24.1	8	44	4.5	41	10	20.0
9	37	5.4	42	8.3	24.1	9	41	4.9	42	9.7	20.9
10	33	6.1	43	8.4	23.8	10	38	5.2	43	9.3	21.5
11	31	6.5	44	8.5	23.5	11	33	6.1	44	9.2	21.8
12	28	7.2	45	7.0	28.6	12	31	6.5	45	8.0	25.0
13	31	6.5	46	7.2	27.8	13	33	6.1	46	7.5	26.7
14	26	7.7	47	7.5	26.7	14	29	6.9	47	7.5	26.7
15	25	8.0	48	6.8	29.4	15	30	6.7	48	7.0	28.6
16	23	8.7	49	6.4[2]	32.8	16	27	7.6	49	6.8	29.4
17	21	9.5	50	6.4	32.8	17	26	7.7	50	8.1	24.7
18	21	9.5	51	6.7	29.9	18	25	8.0	51	6.9	28.9
19	19	10.5	52	5.9	33.9	19	24	8.3	52	6.6	30.3
20	20	10.0	53	5.6	35.7	20	24	8.3	53	6.4	32.8
21	19	10.5	54	4.2	47.6	21	22	9.1	54	5.6	35.7
22	18	11.1	55	4.2	47.6	22	21	9.5	55	6.7	29.9
23	17	11.8	56	3.3	60.6	23	17	11.8	56	5.4	37.0
24	17	11.8				24	17	11.8	57	5.3	37.7
25	18	11.1				25	16	12.5	58	5.4	37.0
26	17	11.8				26	15	13.3	59	4.6	43.5
27	15	13.5				27	15	13.3	60	4.8	41.7
28	15	13.5				28	16	12.5	61	5.7	35.1
29	16	12.5				29	14	14.3	62	3.9	51.3
30	15	13.5				30	13	15.5	63	4.8	41.7
31	14	14.3				31	14	14.3	64	4.9	40.6
32	13	15.5				32	12	16.7	65	4.0	50.0
33	13	15.5				33	14	14.3			

reliable than the others. The averages of the errors in this group are necessarily discontinued earlier than those of the twice a day group; for a given number of days' practice will make the column only half as long as with two periods of practice daily. A number of the subjects did not get started early enough in the semester to complete the learning by the close of the academic year. While this is regrettable, it is to be noted that the records are complete enough for reliable data on the most important matters raised in the following

[1] P. P. = practice period; A. E. = average number of errors in 200 catches; A. C. = average number of successive catches in the P. P.

[2] Averages of this column are 'weighted' below this point, as explained in the text.

discussion of the significance of the curves. Our experiment is only to be regarded as preliminary, so far as affording data for finer points on learning curves is concerned. More data are desirable particularly toward the end of the learning process in the neighborhood of the 'physiological limit.' It will be noted that so far as our averages go in the once-a-day practices, they show more rapid learning for equal practice than do those of the other group with more frequent periods. This agrees with most other experiments on the distribution of practice. Individual variations, however, are so great and the number of subjects so small as to invalidate any conclusion on this point from our data. An unequal proportion of each sex in the two groups is another factor that may be responsible in a measure for this result, though the probability seems to be that a more equal sex distribution would have made the once-a-day results even comparatively better than they were.

In Table III. are also given the average number of successive catches, A. C., in each practice period, the average number of catches without an error. These averages were obtained simply by dividing the average number of errors, A. E., into 200 catches. In order that the successive practice periods might be as nearly equal as possible, the subjects were asked always to stop any series voluntarily, if it did not end there by a miscatch, when the total number of catches of the practice period reached 200.

But this voluntary discontinuance of the final series at 200 catches makes an error in our results which seemed unavoidable so far as the experimental procedure is concerned. This error is fortunately negligible in the first part of the learning process, that with which we are most concerned in this study, since no subject could have exceeded very far the 200 limit by continuing the series voluntarily stopped. As the average number of catches increases this error in our results becomes larger. More specifically, if a subject averaging four catches per series ends by a miss, say, at 198 catches and 50 miscatches or errors, the chances are that he could on the next series reach 202 catches (with 51 errors). Volun-

TABLE III

AVERAGES OF ERRORS IN 200 CATCHES AND AVERAGE NUMBER OF SUCCESSIVE CATCHES[1]

Once-a-day Group						Twice-a-day Group					
P. P.	A. E.	A. C.	P. P.	A. E.	A. C.	P. P.	A. E.	A. C.	P. P.	A. E.	A. C.
1	107	1.8	34	12	16.7	1	97	2.1	34	12	16.7
2	75	2.6	35	11	18.2	2	93	2.2	35	12	16.7
3	69	2.9	36	9.3	21.5	3	74	2.7	36	13	15.5
4	55	3.6	37	9.1	21.9	4	68	2.9	37	12	16.7
5	52	3.9	38	10	20.0	5	55	3.6	38	10	20.0
6	46	4.3	39	8.3	24.1	6	50	4.0	39	10	20.0
7	41	4.9	40	9.1	21.5	7	45	4.4	40	10	20.0
8	39	5.1	41	8.3	24.1	8	44	4.5	41	10	20.0
9	37	5.4	42	8.3	24.1	9	41	4.9	42	9.7	20.9
10	33	6.1	43	8.4	23.8	10	38	5.2	43	9.3	21.5
11	31	6.5	44	8.5	23.5	11	33	6.1	44	9.2	21.8
12	28	7.2	45	7.0	28.6	12	31	6.5	45	8.0	25.0
13	31	6.5	46	7.2	27.8	13	33	6.1	46	7.5	26.7
14	26	7.7	47	7.5	26.7	14	29	6.9	47	7.5	26.7
15	25	8.0	48	6.8	29.4	15	30	6.7	48	7.0	28.6
16	23	8.7	49	6.4[2]	32.8	16	27	7.6	49	6.8	29.4
17	21	9.5	50	6.4	32.8	17	26	7.7	50	8.1	24.7
18	21	9.5	51	6.7	29.9	18	25	8.0	51	6.9	28.9
19	19	10.5	52	5.9	33.9	19	24	8.3	52	6.6	30.3
20	20	10.0	53	5.6	35.7	20	24	8.3	53	6.4	32.8
21	19	10.5	54	4.2	47.6	21	22	9.1	54	5.6	35.7
22	18	11.1	55	4.2	47.6	22	21	9.5	55	6.7	29.9
23	17	11.8	56	3.3	60.6	23	17	11.8	56	5.4	37.0
24	17	11.8				24	17	11.8	57	5.3	37.7
25	18	11.1				25	16	12.5	58	5.4	37.0
26	17	11.8				26	15	13.3	59	4.6	43.5
27	15	13.5				27	15	13.3	60	4.8	41.7
28	15	13.5				28	16	12.5	61	5.7	35.1
29	16	12.5				29	14	14.3	62	3.9	51.3
30	15	13.5				30	13	15.5	63	4.8	41.7
31	14	14.3				31	14	14.3	64	4.9	40.6
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discussion of the significance of the curves. Our experiment is only to be regarded as preliminary, so far as affording data for finer points on learning curves is concerned. More data are desirable particularly toward the end of the learning process in the neighborhood of the 'physiological limit.' It will be noted that so far as our averages go in the once-a-day practices, they show more rapid learning for equal practice than do those of the other group with more frequent periods. This agrees with most other experiments on the distribution of practice. Individual variations, however, are so great and the number of subjects so small as to invalidate any conclusion on this point from our data. An unequal proportion of each sex in the two groups is another factor that may be responsible in a measure for this result, though the probability seems to be that a more equal sex distribution would have made the once-a-day results even comparatively better than they were.

In Table III. are also given the average number of successive catches, A. C., in each practice period, the average number of catches without an error. These averages were obtained simply by dividing the average number of errors, A. E., into 200 catches. In order that the successive practice periods might be as nearly equal as possible, the subjects were asked always to stop any series voluntarily, if it did not end there by a miscatch, when the total number of catches of the practice period reached 200.

But this voluntary discontinuance of the final series at 200 catches makes an error in our results which seemed unavoidable so far as the experimental procedure is concerned. This error is fortunately negligible in the first part of the learning process, that with which we are most concerned in this study, since no subject could have exceeded very far the 200 limit by continuing the series voluntarily stopped. As the average number of catches increases this error in our results becomes larger. More specifically, if a subject averaging four catches per series ends by a miss, say, at 198 catches and 50 miscatches or errors, the chances are that he could on the next series reach 202 catches (with 51 errors). Volun-

tarily stopping at the 200th catch therefore gives him fewer errors than he should have, and correspondingly increases the average number of catches. In this case one half error should have been added to those actually made. Instead of having an average of 4 catches to the series (*i. e.*, 200 ÷ 50) he should really have 3.96 (*i. e.*, 200 ÷ 50.5). If, on the other hand, one averaging nearly 50 catches to the series misses, say, at a total of 175 catches with 4 errors, and then makes up the 200 total and stops voluntarily, he should have not an average of 50 catches to the series (200 ÷ 4), but only one of 44.44 catches (200 ÷ 4.5). In the former case the voluntary discontinuance at 200 made a negligible error in results (1 per cent.), while in the latter the miscatches, 'errors,' should be 12.5 per cent. more than would appear from our method of procedure, and the average number of catches would be 12.5 per cent. less.

Fig. 3 presents in a graphic form the data of Table III., without any correction for the error just pointed out. Curves *E*, I., and *E*, II., represent the number of errors (miscatches) in the 200 catches of the one-practice-a-day group and the two-practices-a-day group, respectively. One practice a day seems to be a slightly more effective distribution than two a day. It is important to note—and this is one of the main contributions of this paper—that these curves are the so-called *normal curves of motor learning*, showing a rapid initial decline and a more gradual decline later, as is the rule with such curves. That this is not evidence of rapid initial learning is obvious from the fact that the other curves in this figure, *A*, I., and *A*, II., representing the average number of successive catches without a miss, have a different form altogether though they represent the same learning process as the *E* curves. That is to say, the progress of the one-practice-a-day group is represented by the curve *E*, I., and also by the curve *A*, I. If we consider only the former we are in danger of concluding—as has been the practice among practically all psychologists for error curves up to this writing—that the learning begins at a rapid rate and gradually decreases in absolute amount in the successive practice

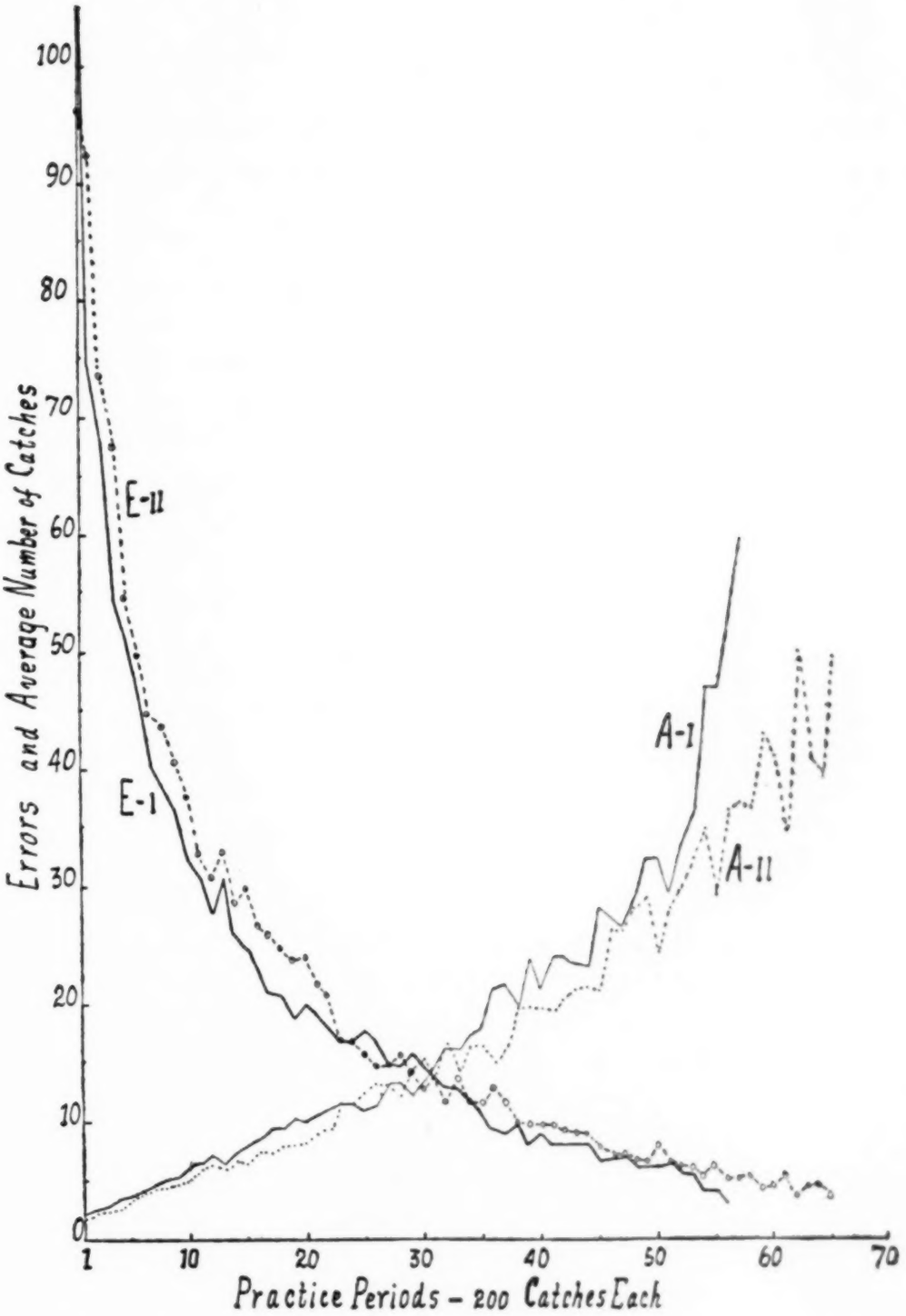

FIG. 3. *E*, I. and *A*, I. are the error curve and the curve of average number of catches per practice period of thirteen subjects practicing once a day. *E*, II. and *A*, II. are the corresponding curves of twelve subjects practicing twice daily.

periods, being very slow toward the end of the experiment. On the other hand, if we consider only the latter curve, just the reverse of this condition will seem to be true. The only fair conclusion, taking both curves into consideration, would seem to be, so far as the data considered are concerned, *that we do not know anything reliable at all concerning the kind of learning in question, as to whether it is initially more rapid or more slow than at certain later stages.* It would seem safest to assume that in the case of ball-tossing, at least, learning goes on throughout, up to a point approaching a physiological limit, at about a constant rate. As we shall see later, there is good reason to suspect that the same thing is true of other learning processes. There is likely no real difference in this regard between the so-called motor learning and the sensory discrimination learning in animals. The distinction usually made between these 'kinds of learning' has long seemed artificial and erroneous to the writer, but only recently, in working up the results of the present experiment, did he secure a good basis in fact for the view that the distinction is incorrect. Every case of motor learning must involve some sort of 'sensory discrimination,' and conversely, all sensory stimuli must come to have some sort of motor significance.

One remarkable result of our experiments is the almost complete absence, in the individual as well as in the averaged results, of any important plateaus. As we shall see later, this may not mean that there were no actual plateaus in the learning, but it may indicate that certain kinds of curves tend to conceal plateaus. The very fact, however, that some kinds of curves tend to show plateaus where there are no plateaus by a different statement of results, or at least no marked plateaus,[1] may tend to cast doubt on the whole doctrine of plateaus in learning.

[1] A degree of evidence for this statement comes out in subsequent pages. Plateaus are shown best in the error or the time curves if they occur early in the learning process; if they occur in the latter part of the learning they show best in the average attainment curves, but are practically concealed in the error curves. See Fig. 10, for example. Some methods of plotting show them much better than others. If, moreover, the average attainment curve (which in ball-tossing seems to be concave to the *y*-axis) has a rapid initial rise because of actual rapid initial *learning* a plateau may seem to

An experiment of the kind here reported, taking a considerable amount of time daily from each of a large number of subjects, makes it impossible, under the conditions of full-time teaching by the experimenter, to keep as close check on each student as is desirable. Even if the experimenter were entirely free from other duties it would be very difficult to get the time of a great number of subjects regularly for the number of months that this experiment requires. Closer personal supervision than was possible to the experimenter in the present case might do something toward reducing individual differences, and especially toward explaining the causes of certain marked failures to learn.

In the introspections, observations, and explanations of the irregularities many interesting points respecting difficulties and the methods of their solution, or failure of solution, came out. These are perhaps not different from those already reported in previous studies, and need not detain us here. One of the very best psychology students of the subjects failed utterly to get a fair start, even with a little personal assistance—unfortunately somewhat too late—from the experimenter. She had had almost no experience in throwing and catching balls, and could not, after the day she came for special help, break up in the time available a confusion of impulses which blocked one another much as in the attempts of a stutterer to talk. Frequently she could not even throw up the first ball and hold the other one, but would throw both together in a spasmodic manner making them go in different directions. After a few weeks of practice she was seldom able even to catch the first ball tossed. The effort was then discontinued, and her results are not included in the data of our tables.

Another subject, *A*, whose results are not included in the averages because they did not reach a point of even fair efficiency, advanced rather slowly after a short initial gain, and at the point of discontinuance the process of learning

occur between this rapid rise in the curve and the one that occurs in the latter part of the curve, due purely to the *mathematical relations* of errors to average attainment. (See p. 199.) Such plateaus are not *plateaus in learning* at all. Just how marked a plateau is, is not easy to determine absolutely.

came to a condition approaching a plateau. This may have been due to the development of some such confusing tendencies as referred to in an extreme form in the preceding paragraph. This subject did not get time to complete the process up to a point that would indicate the real nature of this difficulty. The curve in question is given in Fig. 1. It should be noted that the learning had not come to a complete standstill. *A* was also a student making a good grade in the psychology course; she had had but little experience at ball playing in childhood. Subject *V* is another woman of whom the same things may be said, but her record was carried out to the 134th practice period (26,800 catches!), at which point she still made four errors in the 200 catches. All those who made extremely bad records were women, though some of the women subjects did exceptionally well. On the other hand, some of the men who had had extensive practice at playing ball advanced at an exceedingly rapid rate. Subject *S* is the best example in our groups. / On the whole it appears that men are more rapid learners in ball-tossing of the kind practiced in the present experiment than are women, though our data are not sufficient, in view of the degree of individual variation found, to justify a detailed comparison or any definite conclusion on the matter. For the aid of any one who may desire to test this aspect of the problem out further and who may find use of our own data it may be added that the following subjects were women: *A*, *B*, *C*, *F*, *G*, *H*, *I*, *J*, *K*, *N*, *P*, *V*, *X*, *Y*, *BB*. Since the results averaged in the once-a-day group are of five men and eight women, and those averaged in the twice-a-day group are of eight men and four women, it is possible that the superiority of the former group would be greater with a more equal distribution of men and women. Since the time for practice had to be determined by the convenience of the subjects a lack of balance of this kind was not easy to avoid. It may be suggested here, too, that the very poor records of some of the women may not be due simply to the lack of practice at ball playing in childhood; on the contrary the failure to take up with this popular kind of sport may have been due

to an innate unfitness for it which likewise unfits the subject for ball-tossing.

The reports of the subjects show various individual differences in procedure, such as differences in methods by which the balls are tossed, in the methods of rotating them in the air to prevent collisions, in the distribution and frequency of 'bad days,' in the degree of correlation between feeling of efficiency and actual efficiency as shown by results (this correlation on the whole being very low), and in various other respects. A complete analysis of these differences with the data at hand would contribute but little to previous work, and is somewhat aside from our present purpose.

Statements like the following from *D*, which statement, by the way, came spontaneously from a number of the subjects, show the value of extensive experiments of this kind for students being initiated into scientific psychology. *D* writes: "I now feel as if I should like to continue something of this kind and gradually become proficient in several things. This experiment has surely taken the place of several books so far as learning is concerned, and I am very glad that I have had the opportunity to spend whatever time I have spent on it." The apparatus is simple, the directions are not hard to follow, and the working up of his own results is a most instructive exercise to the student.

We recur now to the error in results pointed out above.[1] Correcting for the effects of voluntarily stopping at the 200th catch would make the average attainment curves—those representing the average number of catches—appreciably less concave to the *y*-axis than is shown in Fig. 3; *i. e.*, more nearly approaching a straight line. In the averaged results of all the subjects it would make a difference of .5 per cent. in the first practice period; of nearly 1.5 per cent. in the tenth; of about 2 per cent. in the twentieth; of about 3.5 per cent. in the thirtieth; of about 5 per cent. in the fortieth; of about 7 per cent. in the fiftieth; and of nearly 10 per cent. in the fifty-sixth. In general it would seem fair to assume that the last series, which is stopped voluntarily each time,

[1] *Supra*, p. 197.

varies at random all the way from zero catches to the number of successive catches possible at any given stage of learning in question. We ought therefore to count, on the average, one half error for this series voluntarily stopped. Adding $\frac{1}{2}$ to the average of the recorded miscatches (errors) and dividing the resulting number into 200, we get the corrected average number of catches per series. A corrected table of the averages of the two groups of twenty-five subjects given separately in Table III. is given below (Table IV.). The formula used for determining the values A.C. in this table is

$$\frac{K}{e + \frac{1}{2}} = a.$$

This is a better empirical formula for conversion than that given above.

TABLE IV

A TABLE OF CORRECTED VALUES OF a AND e; RECORDS OF ALL SUBJECTS OF BOTH GROUPS ARE AVERAGED TOGETHER (28 SUBJECTS)

P. P.	E.	A. C.	P. P.	E.	A. C.
1	102.5	2.0	29	15.5	13
2	84.5	2.3	30	14.5	14
3	72.0	2.8	31	14.5	14
4	62.0	3.2	32	13.0	15
5	54.0	3.7	33	14.0	14
6	48.5	4.1	34	12.5	16
7	43.5	4.6	35	12.0	17
8	42.0	5.0	36	11.6	17
9	39.5	5.1	37	11.0	18
10	36.0	5.6	38	10.5	19
11	32.5	6.2	39	9.6	21
12	30.0	6.7	40	10.0	20
13	32.5	6.2	41	9.7	21
14	28.0	7.2	42	9.5	21
15	28.0	7.2	43	9.3	21
16	25.5	7.8	44	9.3	21
17	24.0	8.4	45	8.0	25
18	23.5	8.5	46	7.9	25
19	22.0	9.1	47	8.0	25
20	22.5	8.9	48	7.4	27
21	21.0	9.6	49	7.1	29
22	20.0	10	50	7.8	26
23	17.5	11	51	7.3	28
24	17.5	11	52	6.8	30
25	17.5	11	53	6.5	31
26	16.5	12	54	5.4	38
27	15.5	13	55	6.0	34
28	16.0	13	56	4.9	41

FIG. 4. Corrected error and average-number-of-catches curves of twenty-five subjects.

Fig. 4, giving graphically the data of Table IV., shows that the concavity of the *a*-curve to the *y*-axis is not removed, though it is somewhat reduced, by the correction described. If the averages could be reliably carried further than to the fifty-sixth practice period—which they cannot because of some subjects 'falling out' with incomplete records—the *a*-curve would rise comparatively much more rapidly as the *e* values become very small; it would reach 400 when the actually recorded errors equalled zero. From this point the fraction, $\frac{1}{2}$, added to *e* should gradually decrease to zero, bringing the *a*-curve up to infinity. If fatigue were eliminated by the subjects' resting occasionally, it is possible that this point on the *a*-curve would be reached (theoretically) early enough to give the curve a decidedly concave aspect to the vertical axis. In the figure in question the *a*-curve agrees with this view by taking rather suddenly a marked upward trend. It would be interesting, with a large number of subjects practicing up to a high degree of perfection, to trace the trend of this curve out further, and to construct a rational mathematical curve to represent it as the 'normal curve' of average attainment in ball tossing for the value of $K = 200$.

Now since $K = ae + \frac{1}{2}a$, it is obvious that the constant practice in successive periods can be measured accurately neither in the number of catches alone nor in that of throws alone irrespective of catches. Since *e* begins with a large value and decreases gradually towards, but never reaches, zero, more practice is obtained in early than in later periods if a practice period is defined as 200 catches (as is done in this experiment). There is reason to believe, however, that the additional practice in early over later periods is small, if not actually negligible in amount.[1] Throwing a ball so

[1] It should be noted that less practice in the early practice periods than was given in the present experiment would have a tendency to make the average attainment curve rise more slowly and the error curve drop less abruptly, making the former curve therefore more concave to the vertical axis than it is. A good way to test the effect of the excess practice in early periods is to have a large number of subjects take practice periods of 200 tosses, irrespective of the catches, and then to arrange the results into successive groups of 200 *catches* each and compare the curve with those in Figs. 3 and 4, which represent averages by our method. Some procedure that is a mean between these two opposite extremes in method would doubtless be the best one.

that it cannot be caught likely gives but little practice in throwing one so that it can be and is caught. Moreover, the method here followed has been common in animal learning, and in all other experiments in which *something is to be done* once or more times for each practice. In all such experiments the constants, or 'equal amounts of practice,' set for successive periods are not strictly equal. Just how much this inequality affects results in any given case is not easy to determine. The difficulty arises from the fact that as practice advances errors and irrelevant and confounding movements are constantly on the decrease while the successful or correct movements are increasing in frequency and in rapidity of performance. Equal time periods of practice has been suggested and extensively used.[1] While this practice has many advantages it also shares the disadvantages here pointed out. A skilled juggler may toss and catch the balls two dozen or more times while the beginner fumbles about with possibly only one successful catch. Are such practice periods equal?

But the greater error comes not from the inequality of practice as such, but rather in the statements of results. This is a most deceiving thing. Total work performed, or average amount of work without (an arbitrarily defined) error, or with a given number of efforts, or within a fixed amount of time, becomes the unit and is represented usually by the ordinates. Abscissa units usually stand for successive practice periods however defined. Such definitions of ordi-

It is, of course, obvious that if our method errs in the direction of excess practice in early practice periods, that of equal tosses errs the other way in as much as wild tosses doubtless give less practice in throwing-and-catching than do tosses which result in catches. Our own method, as has been pointed out in the body of this paper, is more in harmony with the usual practice, and it also gives equal results—200 *catches*—in the several practice periods. My friend and colleague, Professor W. S. Miller, of the department of education, has kindly helped me to convert the data from one of his representative subjects by the equal-number-of-tosses method into average attainment for comparison with my own curves in Fig. 3. Only about the first half of the data from this subject were thus converted. So far as we went no noticeable difference from our own averaged results appeared. His practice periods, however, were 300 tosses instead of our own 200 catches. So the results are not strictly comparable. It would seem that the inequality of different practice periods by either method will show little effect in results so far as the mere practice differences are concerned. But see the next paragraph in the text.

[1] *E. g.*, Thorndike, *op. cit.*, 295 ff.

nate values presuppose a homogeneity of the data measured; but as has been hinted above no such homogeneity exists in the majority of cases. Practice in the early part of the experiment is vastly different from practice in the later part, or, stated differently, no account is taken of how the attainment measured is conditioned reciprocally by 'errors,' of whatever nature they may be. This difficulty is so great, it seems to me, as seriously to invalidate the conclusions arrived at as to the different absolute rates of learning in different stages of the learning process.

In ball-tossing one might define a practice period in terms of *throws*, say as 200, 300, or 400 throws, thereby avoiding the difficulty suggested above. But this does not really avoid it, for all throws are certainly not of equal practice-for-throwing-and-catching value. In the second place one is deceived by such a method when one comes to the definition of results. In one case, not yet published, of which the writer is aware 'the score,' where this method was followed, is the number of catches. Not only is it wrong to assume that a bad throw—such as the beginner is prone to make—gives as much opportunity for a catch as a good throw; but it is even more fallacious to count only the *absolute* number of catches as results, since the *ratio of catches* to the *possible number of catches* is constantly changing. Such results will always, it would seem, give, deceptively, a rapid initial rise in the curve and negative acceleration later. This is due to the fact that out of, say, 200 tosses only 200 catches are possible, and as the 200-catch limit is approached the proportion of catches to possible-catches approaches a limit of 1 : 1. The influence of errors (both of throwing and of catching, if they can be successfully distinguished) is therefore not only a variable quantity directly affecting the catching, but reciprocally and indirectly the number of errors determine the ratio of actual to possible catches. From the very nature of the conditions the limit (in terms of the score) is physically imposed and it must be approached asymptotically. We are bound under such conditions, because of this relation to the errors not being taken into consideration,

to get a curve of attainment which resembles the 'normal' learning curve! All of which is only another way of stating the fallacy of counting results, or the score, in absolute terms of attainment, or of errors, only.[1]

It should be noted that *error* as defined in the present experiment—a miscatch ending a series of catches—is practically the same as *trial*. It is perhaps more clear that results should be equal to the number of trials *times* the average amount of attainment per trial, than that they equal the 'errors' *times* the average amount of attainment. The reason that the term error is used here is that it is in common use in similar relations in psychological literature and that each trial ends by an error, or miscatch. It is also desirable not to confound trial with single tosses, or throws. Counting results in terms of single throws is liable to result in curves of a deceptive nature, as explained in the preceding paragraph, curves which seem, wrongly, to indicate that *learning* is at first comparatively rapid and that it later approaches a limit very slowly.

Because of certain widely current statements about the absolute rates of learning in its various stages, it seems worth while to carry this question of the significance of certain learning curves, raised in the ball-tossing experiment in particular, over into the more general field. Here prudence suggests, in view of the time that has been available for this study, that we proceed very tentatively. The aim is therefore rather to open up the problem and to stimulate further experimentation than to give it any appearance of finality. If anything like dogmatic statements appear, this is to be explained on the ground of brevity and of the belief

[1] The results of Mr. Batson, published in a recent *Psychological Monograph* which appeared after the present paper was sent to press, confirm the general position here taken. Mr. Batson has plotted his results in the three different ways discussed in this paper, and gets for error curves the rapid initial decline usual with such curves; for what we have called average attainment he gets curves which are concave to the vertical axis. Plotting a curve of the number of catches in successive practice periods with a constant number of *throws* he gets a curve which in general shows a rapid initial rise and a final asymptotic approach to the "physiological limit," or better to the 1 to 1 ratio. He does not, however, enquire into the meaning of the different kinds of curves.

that they may be more effective than highly guarded suggestions in bringing about this result.

An Examination of the Nature of Learning Curves

Why do our curves in this experiment, representing errors on the one hand and the average amount of accomplishment on the other, seem so contradictory? Both of them represent kinds of data (results) frequently used as bases of inferences as to relative rates of learning in the different stages of the process. Moreover, error curves and attainment curves are frequently compared directly without any kind of transformations. As has been pointed out in the specific cases considered, errors and average attainment have reciprocal relations to each other as dependent variables. Each factor conditions the other in such a manner that it is entirely unsafe to define learning in terms of absolute units of only one of these processes. In most cases, as was found true in ball-tossing, time curves and error curves have the same general form, so that the former have the same relations to the curves of average attainment that the latter have to them. In certain problems, such as maze learning, excess distance curves show the same general nature as time and error curves.[1] Of course, in these various cases 'errors' are not the *same kind of things*, even though they may be safely comparable. Much depends upon how they are defined, as is well known.[2] On the whole more errors means more time taken for a certain result in terms of performance; the same thing is true in a rough way of the relations of excess distance to performance or attainment. For the general purpose of this enquiry, then, we may confine our-

[1] Cf. Hicks, Vinnie C., 'Relative Values of the Different Curves of Learning,' *J. of Animal Beh.*, 1911, *1*, 138–156.

[2] This is true even within the same kind of experimentation. For example, in the case of animals learning the maze an error may be defined as merely putting the head into the *cul-de-sac*, as entering it with the full length of the body, or as going the full length of the *cul-de-sac*. A return in a single section without a bend may also be counted as an error along with any complete return, or the latter may be counted for as many errors as there were sections passed through in the return, or, a third possibility, no returns may be regarded as errors if the animal does not depart from the true path.

selves to error curves, keeping in mind that, with certain cautions, time or excess distance curves may be substituted for them without serious error.

The reciprocal relations of errors and average attainment, as we have defined these terms in ball tossing, are obvious. An error always ends a series. When the error count is large the average number of successive catches (the 'series') must be small, and conversely. Suppose that on the average only one catch can be made in a trial, or a series, at the beginning of the learning. Then the errors in a practice period of 200 catches must amount to 200. But an improvement or gain of one in each series (making an average of two successive catches per trial) will bring the errors down to 100, a change of 100 absolute units. An average of 3 catches each trial brings the errors to 66; of 4, to 50; of 5, to 40; and so on. If we plot these values on an absolute scale, representing errors by e and average number of catches by a, we get a 'representative learning curve' of the e-values when the a-values advance by equal absolute amounts. That is, the e-curve drops rapidly at first and then progressively less and less. This is of course on the assumption that we give these values the relations indicated in the equation

$$\frac{K}{e + \frac{1}{2}} = a,$$

and regard K as a constant. Very small changes in a will mean very large changes in e, and *vice versa*. Proportionately, of course, the changes are equal in both factors; *but learning curves are almost universally plotted in absolute amounts*, despite this fact. In the beginning of any learning process the values of *a must be small;* hence *e will always change rapidly on the initial part of the curve* thus plotted. When a gets rather large, e must change slowly, if the changes in a, absolutely, are constant. These changes in the error curve are obviously in no sense indications that the *learning* is at first rapid and that it then gradually decreases in rate.

Let us take a theoretical case of ball-tossing and assume that the increase in the average number of successive catches

—the *a*-values—is constant, adding one unit for each successive practice period. In such a case the *a*- and *e*-curves will have the forms shown in Fig. 5, when $K = 200$. When K is given a value of 40, the *e*-curve takes the form of e_1 in Fig. 6. The curves e_2 and e_3 correspond to gains in the

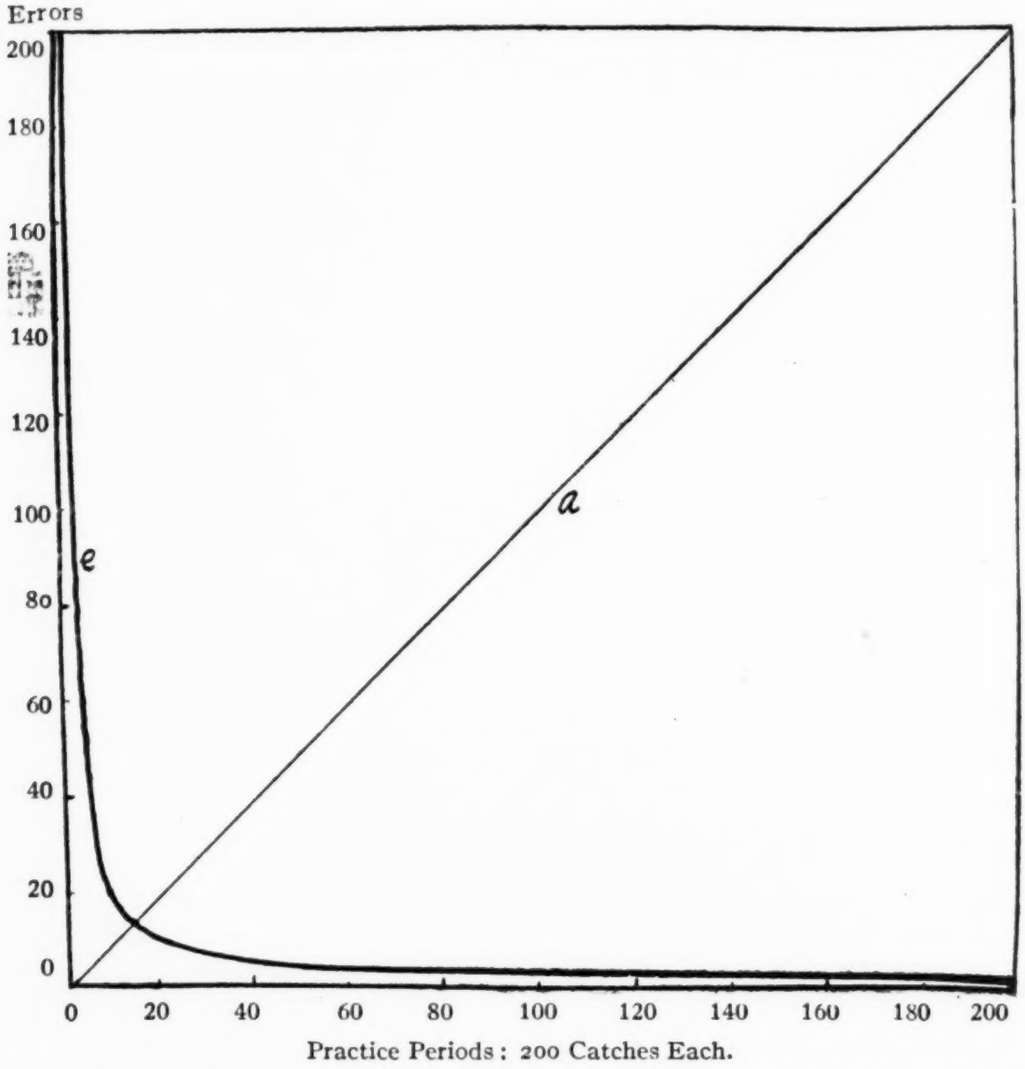

FIG. 5. Curve *e* is a theoretical error curve corresponding to an increase in *a*, average number of catches, of one catch in each succeeding period, when $K = 200$.

a-values of $\frac{1}{2}$ and $\frac{1}{4}$ units, respectively, in successive practice periods. These *e*-curves all have the general shape of the 'normal curve of motor learning,' falling off rapidly at first and more and more slowly as the learning reaches an advanced point. This type of curve is *normal* to motor learning

only because errors, and number of seconds (time), are the easiest data to get in such processes! This unfortunate circumstance, as it has proved to be, a mere contingency, seems to have led to a misapprehension of the real nature of learning. The relative rate of learning at its different stages has come to be judged, in the hands of not a few investigators, by the rate of decline of this curve. It should be noted that the smaller the values assigned to K the more gradual the decline of e. Relatively slow, constant gains of the a-values also affect in a similar way the e-curve. A rapid initial rise of a produces, of course, a more abrupt fall of the e-curve, as shown in Fig. 7, curve e.

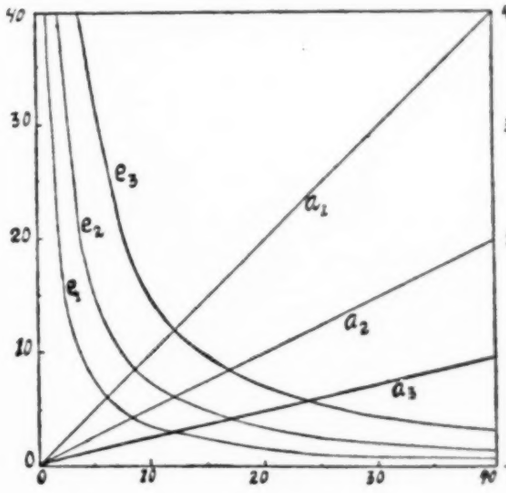

FIG. 6.

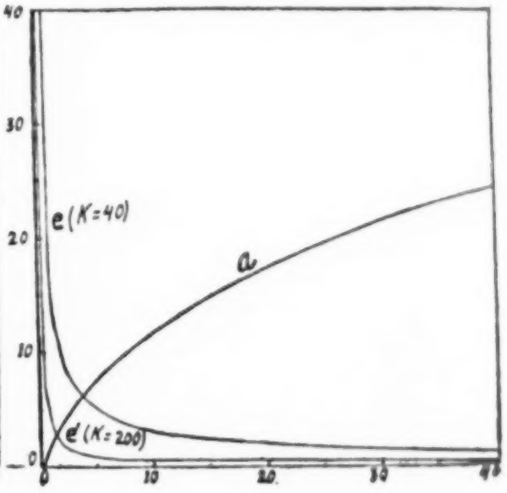

FIG. 7.

FIG. 6. e_1, e_2, and e_3 are theoretical error curves corresponding respectively to equal absolute successive gains of one, one half, and one fourth in the average numbers of catches, a_1, a_2, and a_3. $K = 40$.

FIG. 7. e and e' are theoretical error curves derived from the average attainment curve a, when $K = 40$ and 200 respectively.

The *form* of the a-curve is a third factor that affects the reciprocal e-curve. If, for instance, a begins with a value of 1 and increases in each succeeding period by 1/10 of its value in the next preceding period, we get curves like those shown in Fig. 8, when $K = 200$. These curves resemble very closely those actually obtained in ball-tossing by certain subjects. Whether this type of a-curve, representing *equal proportional gains*, comes nearer being a constant progress in

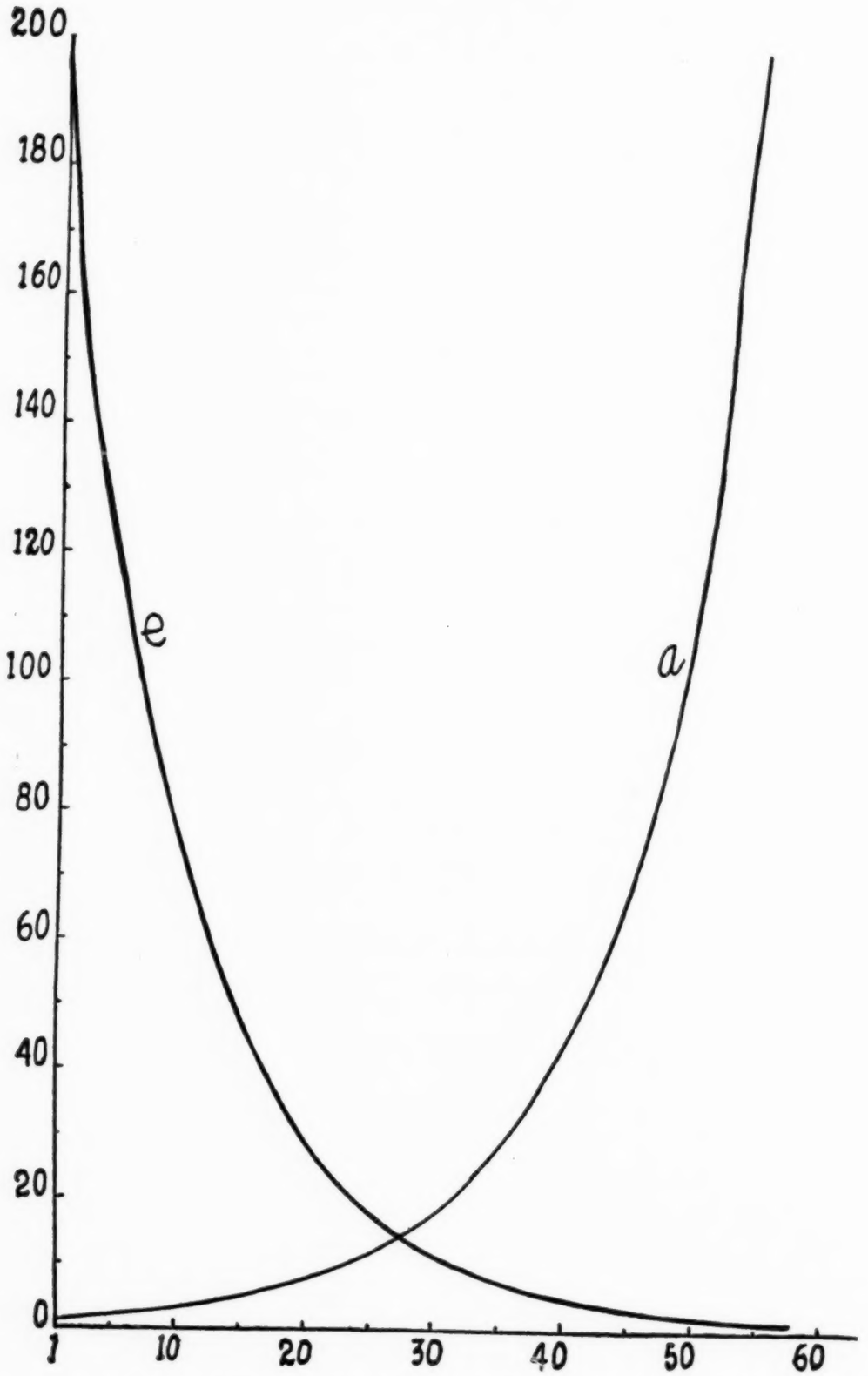

FIG. 8. Curve *a* represents equal proportionate gains of 1/10 over each preceding period, the initial value being 1. *e* is the corresponding error curve when $K = 200$.

learning than equal absolute gains of the *a*-values is a question that may be answered differently by different authorities and by the same authority for different learning processes. At any rate when plotted in absolute units the curve of such a gain is concave to the vertical axis, as are the average attainment curves of ball-tossing. This correspondence might suggest that learning in ball-tossing advances by approximately equal proportionate gains; but to take this stand would be to get into a questionable position, to make the mistake with a slightly different application which has been made to date by certain psychologists in the interpretation of error and time curves. When the average attainment is great the errors are few, and because of the reciprocal relations of *a* and *e*, pointed out in the foregoing, *slight changes in errors must mean large changes in average attainment*, when these values are plotted in absolute units.

If the *a*-curve has an initial rapid increase, as shown in Fig. 7, making a convexity toward the vertical axis, the initial drop of the *e*-curve is increased in amount. If the K in our equation, by which the *e*-values were derived, is decreased in value the initial drop will decrease correspondingly. (Compare curves *e* and *e'*, Fig. 7.) It is of course obvious that any tendency to a rapid initial increase in average attainment, as defined in this paper, will correspondingly increase the decline of errors, or trials.

When we consider these relations of curves of different kinds of data obtained in learning it becomes evident that great care in the comparisons of different learning curves is advisable. All 'errors' and 'scores' are more or less arbitrarily defined, and it seems necessary for comparative purposes that some standardized methods of conversion of data be devised. At present we are forced to recognize the insecurity of any conclusion as to the absolute rates of different stages of learning, based on such curves of learning as are available. As a rule curves of learning are based on the total amount of accomplishments of the kind specified as 'results,' but in many cases they are constructed on the basis of what *can be done* in a stated number of minutes

after certain multiple units (weeks, hours, or minutes) of practice. In such cases the initial stages do not allow of as much practice *in the particular aspect of learning that is plotted* as do later stages, for more time absolutely is lost on various conditioning functions. The considerations of some of these matters may even result in the view that a relatively more rapid initial learning takes place in certain cases than is commonly assumed from the nature of the curves of the data obtained. On the whole, however, the evidence for the general statements current, that learning is initially rapid and gradually decreases in rate to a physiological limit, is far from satisfactory. Meanwhile it must be recognized that the practice of putting into graphic form the 'progress' in different learning processes studied, has had fruitful results toward a more quantitive psychology of learning and toward the establishment of a better knowledge and control of various aspects of learning in educational fields. It may be asked, May we not still continue to plot curves of various learning processes and to say certain definite things about them? Yes; provided we are careful as to *what* we say, and as to what comparisons we make. The curves are not the learning process and in most cases they represent it but poorly. For example, plateaus may not be plateaus *in learning* at all; we may be plotting some aspect of the process which for the time is neglected for some other aspect. On the other hand, a real plateau on the latter part of an error curve, where change is slow, may be practically obscured, while it would have been much more conspicuous in another type of curve.

Some Applications and Suggestions

In every case of learning scientifically studied some definite units of practice must be decided upon—food must be obtained once or several times by an animal either escaping from or getting into a problem box, or by one finding its way past a certain number of blind alleys to the food box in a maze; a given number of catches of balls, of additions of numbers, of throws of javelins, of substitution of symbols,

or of distribution of cards must be made; or, a more comparable and constant procedure in many cases, a given number of minutes or hours must be devoted to a certain specified kind of practice. Whatever the unit of practice may be we may designate it, with certain cautions, by a constant, K.

The score, or results, at any stage of the learning may be determined in at least two, and probably only two, general ways. First, the number of errors (or trials)[1] made in performing the 'work' designated by the constant—such as errors in getting to the food, in making 200 catches of the balls, in striking within certain limits in javelin throwing, in adding certain groups of numbers, in tracing a star, etc.—may be recorded; or the number of seconds required for some such performance may be indicated;[2] or we may in the maze problem take the excess distance covered. In the second place, we may choose the reciprocal results, the amount of performance in certain units (determined by the nature of K) which can be reached on an average without an error or in a given number of seconds. The first of these classes of data on results will give what we have called e-curves; the second, what we have in a general way spoken of as average attainment curves. In the following very tentative comparison of curves taken almost at random from various important experiments on learning by different investigators we shall try to keep distinctly in mind these different factors. In doing this we in no way claim for our classification and formula superiority over any others that may be proposed. We shall use K always to designate the constant of practice; e, the errors, number of seconds (time), or excess distance in maze; a, the amount of performance, attainment, or average attainment. For the transformations of the data from various experiments we shall take some form of the

[1] See *supra*, p. 205.

[2] In experiments on typewriting, telegraphy, crossing out letters or numbers, etc., an error may be defined as failure to make two successive strokes within a given fraction of a section. This would not be greatly unlike our errors in ball-tossing, since too much attention to the case at hand would make an error in the next one. Here again time and error curves show close relationships.

equation,

$$\frac{K}{a} = e,$$

to suit the particular conditions of the case in question. Our purpose is to attempt schematically, under some one of the two classes of results defined, a comparison of data from different experiments. It is obvious that different values of K, whether stated numerically or otherwise, and different arbitrary definitions of e, particularly when e designates errors, will wholly determine the values of a and therefore the fruitfulness of our comparisons.[1] Our purposes are, however, chiefly to illustrate a procedure that is possible if the various experiments are originally planned for comparative purposes.

In most reports of experiments only two of our factors, K and either e or a, are given, and from these the other one—even if only a theoretical factor—may easily be derived by the simple arithmetical process indicated by our equation. We may thus in a general way compare different 'kinds' of learning and see whether there seems to be so much difference as has generally been supposed, or we may at least suggest a method for comparisons in future experimental work. In our illustrative cases considerable difficulty has been experienced because of the fact that data were not fully given in all cases. To facilitate comparisons we will make all e-curves represent decreasing values and a-curves increasing values. This will require in some cases simple transformations of some of the authors' results before the application of our equation is made, but it will not change their values. In every case the curve represented by a continuous line (———) is the curve giving the original data of the author, and the curve in a discontinuous or broken line (– – – – –) is the one supplied by the present writer through dividing the original values (the a's or the e's) into the constant, K. The broken

[1] That is, even for purposes of comparisons between different experiments of the rates of change in e- or in a-curves, we must, to make our conclusions safe, keep the other two factors of the equation, K and a (or e) approximately equal in both cases. We must also be sure that our e (or a) values compared are comparable.

line therefore represents only another statement of the results supplied by the original author, one, however, which may be more directly comparable with results of other experiments.

Since it is manifestly impossible to rearrange the data from most of the experiments referred to so as to get accurately comparable values of K, a, and e, we can expect only a general conformity of the curves compared. More quantitative comparisons must come from results of experiments deliberately planned for comparative purposes, thus giving the different factors of the transformation equation as nearly equal values as possible. The relative size of the abscissa and the ordinate units is another matter that greatly determines the form of the curve, as is well known; but with unequal values of the factors of our equation it is manifestly incorrect to make these units in the several cases all equal. This makes it clear that our comparisons must be regarded only as illustrative.

In Fig. 9 curve a represents the average number of catches without a miss in practice periods of 200 catches each, by Swift's subject A, and e is the error curve, corresponding to the error curves of our own subjects. The data for the average number of catches was taken from Swift's curve,[1] and it was rearranged into such a form that successive practice periods are equal so far as the number of catches are concerned. As is well known the actual practice periods in Swift's experiments were far from equal. The error curve, whose values were obtained by dividing the successive average number of catches into 200, corresponds very closely to many of our own error curves. The learning of Swift's subjects has usually been considered to differ from the normal type of learning. There is evidently little ground for this view when similar results are compared.

Bryan and Harter's classical receiving curve for learning telegraphy is represented by a in Fig. 10.[2] A corresponding time-curve is represented by e, the number of seconds required for 'receiving' five letters. In this case a difficulty

[1] *Amer. J. of Psychol.*, 1903, *14*, 201–251.

[2] *Ibid.*, 1899, *6*, p. 350. Subject W. J. R.

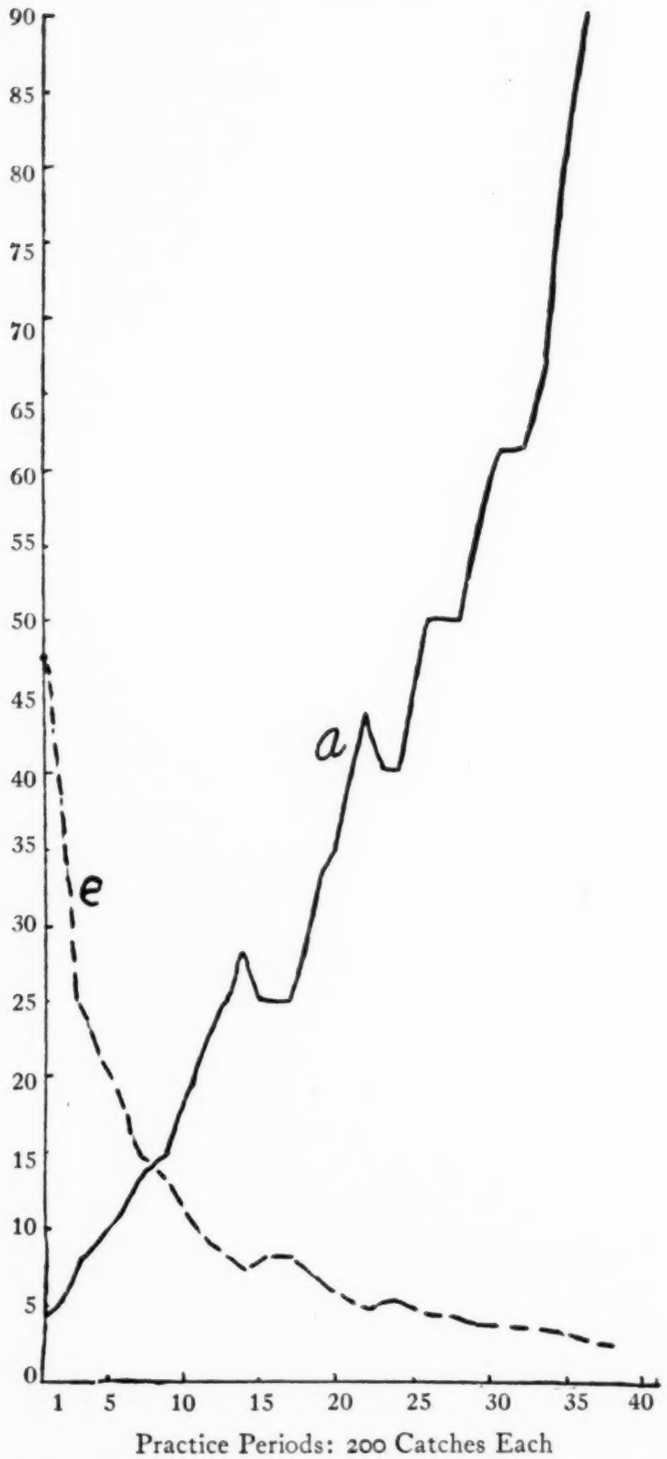

FIG. 9. Error curve, *e*, and average-number-of-catches curve, *a*, of Swift's subject *A*, rearranged into practice periods of 200 catches each.

is encountered. The original curve, *a*, of the authors represents the number of letters per minute *after the several successive weeks' practice*, and is not an average of the whole practice period. It seems probable that with a proper adjustment of ordinate and abscissa units, and with a slightly different tabulation of data—which is impossible for us to

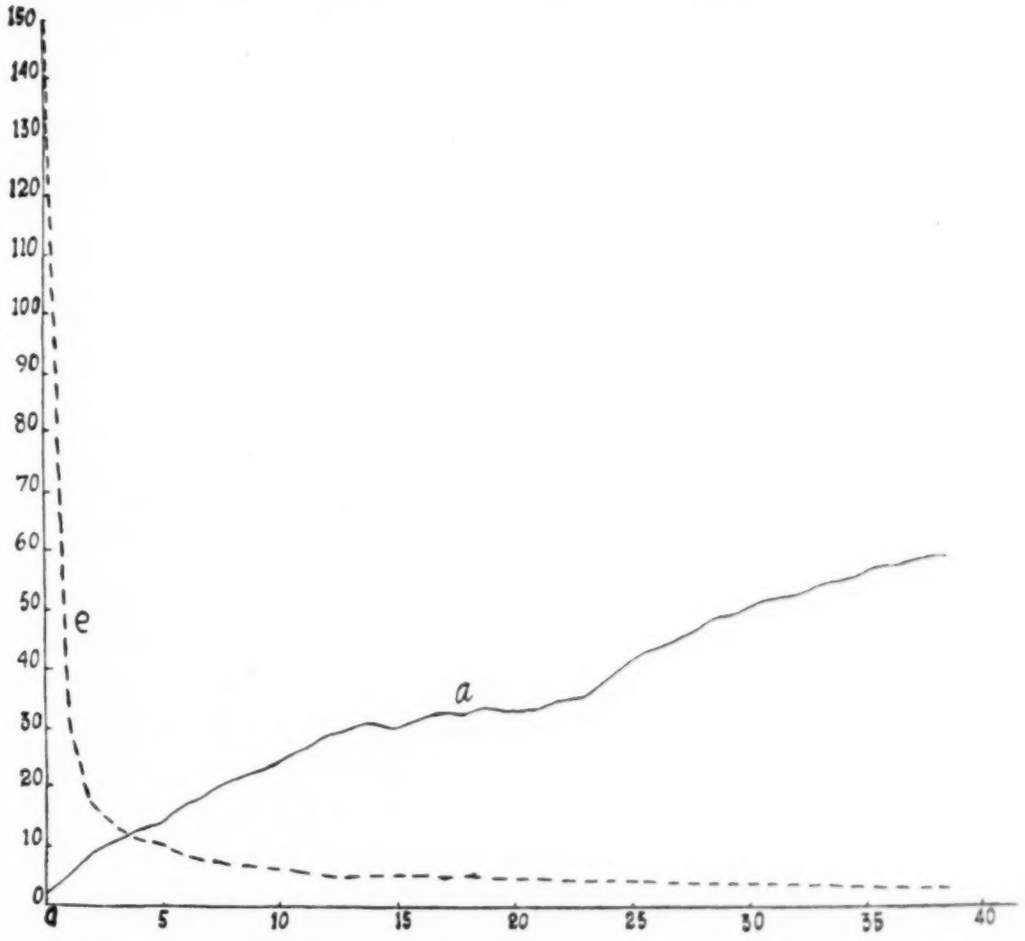

FIG. 10. *a* is Bryan and Harter's receiving curve (number of letters per minute), telegraphy experiment, subject W. J. R. *e* is the corresponding time-curve, representing number of seconds for receiving five letters. Abscissa unit = one week of practice.

show because the complete data are neither given in the original report nor embodied in the curve—we should get the 'normal (time) curve.' The curve here given from such data as was available has an initial drop that is very sudden. Evidently, however, this is the curve that we must compare with other time and error curves, not that (*a* in the figure)

originally given by the authors. It is interesting to note that the plateau, so marked in the original, or average, curve, is not noticeable in the time curve, because it occurs where absolute changes are very small in this curve, on the principle already explained in this report. This may be a valuable suggestion as to the reason why some experiments on learning do not show, and others do show, plateaus. Speculation here without further data, however, is unprofitable.

In animal psychology it has become customary to regard 'motor learning' and 'sensory discrimination learning' as somehow different in *kind*. As is well known, both kinds of learning are frequently represented in terms of errors. But their error curves are markedly different. To quote from Yerkes: "The former [error curves of the labyrinth tests] fall very abruptly at first, then with decreasing rapidity, to the base line; the latter [those for the discrimination test], on the contrary, fall gradually throughout their course. Evidently the labyrinth habit is more readily acquired by the dancer than is the visual discrimination habit. Certain motor tendencies can be established quickly, it would seem, whereas others, and especially those which depend for their guidance upon visual stimuli, are acquired with extreme slowness."[1] Watson, in 'Behavior,' seems to take the same position in general, making different divisions for discussion of sensory and of motor habits.[2] While it is, of course, not to be denied that sensory discriminations may be made very difficult for an animal, so that learning to discriminate will be slow comparatively, it should not be overlooked that the same thing is true of 'motor learning' and that a difference based on sensory and motor habits cannot properly be made. The distinction has always seemed to the writer to be artificial and based on bad psychology. Any habit may be regarded as either sensory or motor, or better, both, according to the point of emphasis. This was long ago made clear by Dewey in his well known article on the reflex arc concept.

One of Yerkes's sensory discrimination curves,[3] modified

[1] 'The Dancing Mouse,' p. 236.

[2] Chapter 6.

[3] *Op. cit.*, p. 230.

to represent increase over 50 per cent. correct responses, as explained in the foregoing, instead of decrease from 50 per cent. error as stated by the author originally, is shown in Fig. 11. Here it is the usual average attainment curve, as might be expected from the data which it represents, even though it is called an error curve.[1] Now, on the basis of

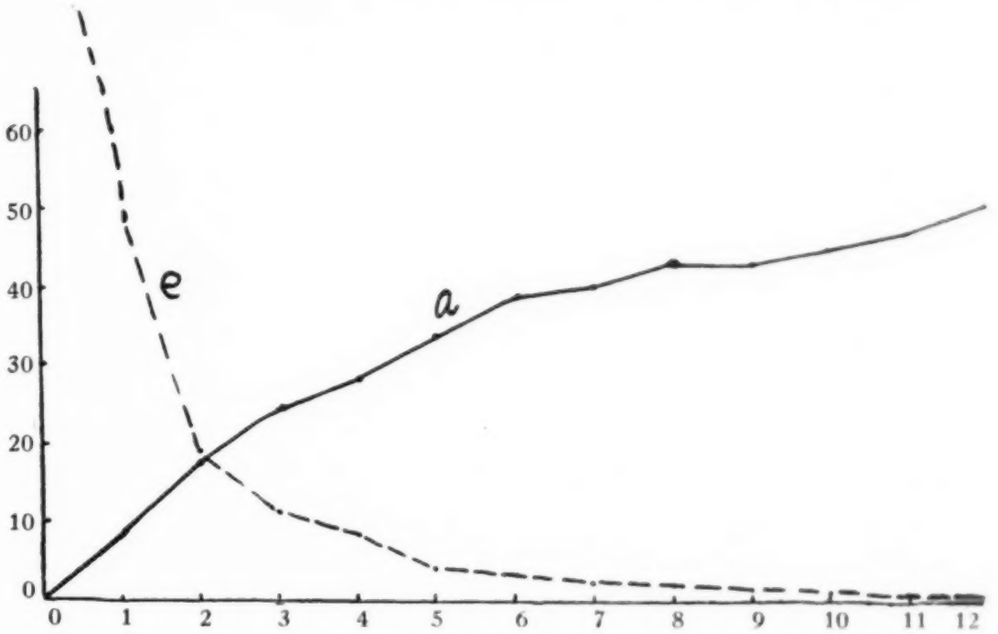

FIG. 11. Curve *a* is rearranged from Yerkes's curve of brightness discrimination by the dancing mouse. *a* represents per cent. correct choices above 50 out of ten trials. *e* is the corresponding (theoretical) error curve derived with a value of $K = 50$.

our equation it is possible to get a value corresponding more nearly to the usual error curve of so-called motor learning. For this purpose we have used the equation

$$\frac{K}{a} - 1 = e,$$

so that the error curve when $a = K$ will reach the zero line, as would actually be the case. Taking $K = 50$, the highest per cent. correct above 50 that is possible, and getting our values of *a* by subtracting the values given in the last column

[1] This statement, it is frankly confessed, needs more study and analysis. It is not easy to see in the present case the values comparable to our *a*- and *e*-values; but the curve given by Yerkes is certainly not an *e*-curve, as here defined, and should not be directly compared with the error curve in the maze learning as is done. If Yerkes had compared it with some sort of attainment curves he could not have come to the conclusion that he expresses.

of Yerkes's table from 50, we obtain the *e*-curve shown in Fig. 11. Whatever this *e*-curve represents concretely in the present discrimination experiment, it is certainly interesting to note that it is almost identical with Yerkes's own error curve of the labyrinth test.[1] Of course, this may not be wholly convincing since the ordinate and the abscissa units are not respectively equal in the two cases, but it is very suggestive of a better mode of comparison of curves than has hitherto been used, one that should avoid some of the errors made in comparisons in the past. It is, of course, always to be admitted that in spite of any manipulation of data one learning process may be made very much more difficult than another. By the use in comparative psychology of some such conversion method as here suggested, however, we may avoid the errors of making misleading comparisons between curves which may chance to be called by the same name and thus conclude that there are different kinds of learning, and we may also avoid erroneous statements regarding the absolute rate of learning at different stages of the process.

Murphy, in a recent article,[2] shows the usual confusion between error (or time) curves and average attainment curves. His subjects practiced at throwing the javelin. Five (?) throws were made by each subject at each practice period, these periods being distributed differently for different groups. Results for each practice period were given in terms of the average distance in centimeters from the bull's eye. "In the above experiment," the author concludes, "the curves did not follow the normal curve of learning to any marked degree; but those in the alternate day group generally gave a better approximation to a regular learning curve."[3] As a matter of fact, Murphy's curves fluctuate a good deal because of the comparatively few trials in each

[1] 'The Dancing Mouse,' p. 235.

[2] Murphy, H. H., 'Distribution of Practice Periods in Learning,' *J. of Educ. Psychol.*, 1916, 7, 150–162.

[3] *Ibid.*, p. 161. Murphy plainly refers to the error, or time, curve as the 'normal curve of learning.' Such curves will be found on examination almost universally, if not quite so (even in spite of the fact that errors are differently defined) to correspond rather closely to our *e*-curve in the foregoing. It is rather surprising that this fact has not received explicit attention before, as it does not seem to have done.

practice period. Some of them, however, show a fair approximation to the average attainment curves usually found. His own results were plainly average attainment. A bunching of his results into more practice for each period—a process impossible to us because he does not give individual results—would obviously give more uniformly declining curves. If, then, the results were stated for successive periods in terms of differences from the largest average distance from the bull's eye (*i. e.*, from an error of 82 cm.), we should have a gradually rising average attainment curve very similar to the usual *a*-curve as defined in this paper. It is unfortunate that Murphy's results are not given in enough detail so that the transformation suggested here could be made as illustrative. To compare curves of average attainment, as Murphy has done, with the 'normal curve' (of error or time) is to make an unfortunate error that is all too common in psychology and education. All curves of different aspects of learning have frequently been regarded as somehow representing the same thing—*the* learning process—however different the data may be which they represent in the various kinds of practice.

In the javelin-throwing experiment it is not at once obvious what corresponds to errors in an experiment such as ball-tossing or the learning of the maze. Let us suppose, however, that every throw missing by more than a given distance, say 10 cm., had been called an error, and that every hit within this limit had been counted a score. Under these conditions if each practice period consisted of some appropriate number of hits (hits within the arbitrary limit) it would seem probable that the results would be comparable in a satisfactory way with those of our ball tossing experiment, for instance. It is true that the correspondence of the data from two such experiments would yet be only approximate in as much as the balls tossed and not caught would give relatively less practice in tossing-and-catching than throwing a javelin slightly outside of the 10 cm. limit would give in throwing-the-javelin-within-the-limit. It must be noted that in our own experiment the successive practice periods are equal in amount of practice only in as far as

throws that are caught are counted *as practice*. Though there is some degree of justification in not counting as practice balls thrown wildly and missed, it is true that many balls are missed through the merest slip in technique, such as letting one ball strike the edge of another when a fraction of a millimeter to the side would have resulted in a catch. Certainly there is practice in such a throw and miscatch and one learns by it, among other things, to avoid such collisions.

In comparing results of different kinds of learning we must exercise considerable care not only to get curves of the same general type, as above illustrated, but also to see that practice is approximately equal in successive periods in each of the experiments compared and that the practice periods of each kind of learning are about equal. The writer hopes to find opportunity in the near future to obtain data of more nearly comparable values in two or more kinds of experiments than exist at present. It must be emphasized, however, that errors resulting from slight inequalities of practice periods are really less serious than those from errors in arrangement of data, some of which have been briefly and very imperfectly pointed out in this paper. It is unfortunate that important comparisons of incomparable data, and that classifications of different 'kinds' of learning have worked into psychological literature and thought with scarcely any attention to this matter of basic import to any reliable comparisons among learning processes in different kinds of performances.

A single result of taking time, or error, curves to signify changes in the learning process itself comparable to changes in the curves may be found in Hobhouse's early reaction[1] to certain statements of Thorndike. Thorndike showed time curves of various animal-learning processes and contended that learning resulted from gradual physiological changes in the nerve tracts. Hobhouse, it will be remembered, objected that the curves in question were in reality steeple-like, the sudden drops being indicative of the influence of something like ideas, or 'practical judgment.' Thorndike did not apparently make the mistake of assuming a corre-

[1] *Cf.* 'Mind in Evolution,' 1901, 145 ff.

spondence between the curves and the actual learning, and his general position was against such an assumption; but he did not explicitly show why the rapid initial drop occurred. He has more recently in the volume already referred to, for example, given various reasons why learning should be more rapid initially than later in the process. These reasons may, indeed, apply in certain cases; but certainly the whole matter needs further experimental investigation. Hobhouse's arguments, so far as they were based on the steeple-like features of the curves, were certainly groundless, though critics of his position have been slow to point out this fact, apparently accepting his own assumption. Numerous statements might be quoted, besides those already given, to show that this has been the usual understanding of the significance of learning curves.

Summary of Results and Conclusions

As a result of experiments on ball-tossing with twenty-eight subjects it has been found that the learning curves of ball-tossing are not different from learning curves of other processes, provided that similar kinds of data be compared. The error curve of ball-tossing, for example, has a rapid initial decline as have error curves—the usual 'normal curve'—of various other types of learning. The curve of the average number of catches per trial, on the other hand, rises gradually at first, making almost a straight line, and more rapidly later. This gives it a concave aspect to the *y*-axis, as has been noted by Swift. Neither of these curves, however, is to be taken as exactly indicative of the relative rates of learning at different stages of the process. Other forms of plotting the results are possible, some of which would seem to suggest that learning is initially rapid and becomes slower as the process advances. Serious errors have resulted in the interpretation of learning from giving too much significance to the forms of any of these curves. On the whole little evidence exists, so far as ball-tossing is concerned, for the current view that learning is at first much more rapid than later when considerable skill has been attained, this despite the form of the error curves. Error curves are

found to be mathematically so related to curves of average attainment that the former must drop comparatively very rapidly at first while the latter change slowly, and, conversely, they must drop very slowly later while the average attainment curves rise rapidly. This is in ball-tossing. The changes described do not mean corresponding changes in *the learning*. There is good ground for the view that the same thing may be true of all learning curves. In general, it appears, curves of errors and of time (and of excess distance in the maze) will, because of this mathematical relation to attainment curves, always have a sudden initial drop. In cases showing a rapid initial rise of the average attainment curves it is probable that learning is initially more rapid, as has commonly been supposed, but the difference in the initial and the later rates is likely not as great as it has been thought to be. Even in such cases, and ball-tossing does not seem to be one of them, the evidence is questionable—the rapid initial progress may in many cases not be one of learning at all, but one resulting from the method of stating the results. The whole subject needs a careful experimental overhauling and methods of making data from different experiments more comparable need to be worked out. A tentative procedure is suggested in the present paper, and a few applications are given as illustrative, but only roughly, due to the incompleteness of reported data by various authors.

The basis on which our present views of plateaus and of the approaches to physiological zero rest is insecure. It is found that whether or not a plateau appears and the degree of its occurrence, depend much on the type of curve chosen to represent it. If, for instance, it occurs late in the learning process it may be almost wholly obscured in the error or the time curve. This may be one reason why error and time curves so seldom show plateaus. When the proper conversion of data is made before comparisons there seems to be little ground for the distinction frequently made between sensory discrimination learning and motor learning in animal behavior. The difference is likely mainly one of the method of calling out the reaction and of stating the score; the learning in both cases can be so stated as to make the two processes quite comparable.

THE SPEED AND ACCURACY OF MOTOR ADJUSTMENTS[1]

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Motor activity must necessarily consist of habitual responses to the same or repeated stimuli, unusual responses to novel stimuli, or modifications of habitual responses to a stimulus partly old and partly new. As it is doubtful whether an entirely novel situation ever presents itself to an adult human being, and as stimuli are seldom repeated in exactly the same combinations or successions, our activity consists for the most part of modifications of our responses to meet changes in the stimulus complex.

The more nearly identical the complex of stimuli, the more nearly identical will be the responses; as stimuli become more diverse, the responses must be more radically adjusted. For instance, when one walks along the street the locomotor stimuli are practically the same. If the steps were accurately measured, however, they would be found to vary in speed, in length, in direction, in the intensity of the foot-fall, in the height to which the feet are raised, and probably in other ways. These minor variations are due in all probability to slight changes in the situation complex such as irregularities in the sidewalk, changes in the position of other parts of the body, breathing, blood flow, glandular activity, etc. A greater variation occurs, or what is the same thing a more radical adjustment is made if one steps on a sheet of ice, comes to a gutter, or is suddenly confronted by another pedestrian.

HISTORICAL

The three directions in which the adjustment of a movement may take place, in the physical sense, are extent, time and force; and these three must work together, a change in

[1] Contribution from the psychological laboratory of Columbia University.

one being compensated for by a change in one or both of the others. These three functions of movement have been adopted by psychologists in their study of bodily movements. The problems attacked have been whether we may perceive time, extent and force independently, or whether one or more are fundamental and the others derivative; and the relative accuracy with which the different factors may be perceived and controlled. (13, Chapter III.)

The experimental work on these problems has been of two sorts. In some of the work the attempt was made to eliminate one factor and study the relations of the other two, while some investigators have let the subject make free movements and have secured measurements of all three. Most of the work has been done in connection with the study of lifted weights, while studies that have had to do primarily with movement have been mainly concerned with the analysis of constant errors.

Müller and his pupils (8, 10, and 11) were primarily concerned with the judgment of lifted weights. They controlled the extent to which the weights were lifted and measured the speed with which the different weights rose and by this method endeavored to analyze the factors essential to the judgment of lightness or heaviness. Besides their findings that have to do peculiarly with lifted weights, they brought out one fact that seems to have a wider application; that is, the fact of a motor set (Einstellung). It is a fact that if a heavy weight is raised a number of times and then a light one lifted, the light one will be lifted much more quickly than were the heavy ones. This fact led them to emphasize time as a factor in the judgment of lifted weights. They showed that if two objects of equal size but different mass, such as 700 grams and 2,500 grams, were lifted alternately with equal speed and to the same height for about thirty times, a set is obtained such that, if for the 2,500 gram weight another is substituted of 850 to 950 grams, it is judged lighter than the 700 gram one. The explanation given was that the effect of repeatedly lifting a light weight followed by a much heavier one is to establish a set of the organism such that greater force will alternately

succeed a less amount. If, after a habit of alternating the amount of force has been set up, a lighter weight is then substituted for the very heavy one, it will be raised with such force that it will rise more quickly than its lighter companion and be judged the lighter.

Other evidence that speed is a fundamental factor in the adjustment of motor response has been brought forward by Jacobi (5). He had his subjects raise simultaneously two weights, one with either hand, and judge which was heavier; the moment each weight rose being recorded graphically on a smoked drum. He wanted to see whether the inertia of the weights was not a basis of comparison. He found that when the weights were judged the same they began to rise at about the same time, but when one was judged heavier it took as a rule longer before it began to rise. When the difference in time was less than 0.08 sec. the two weights compared usually seemed equal, between 0.08 and 0.12 sec. they sometimes seemed equal, but when the time was greater than 0.12 sec. the weight raised last was always judged heavier, whether it was in reality heavier or not. He concluded that the comparison of weights consisted in the comparison of the times necessary to overcome the inertia. Woodworth (13, p. 135) objects that his results were too few to serve as a basis for any statistical correlation and hence lacking in conclusiveness.

Experiments with the size weight illusion also show that inertia as well as the height to which the weights are lifted feature in the judgment. Claparede (2), experimenting with this illusion, used three cubes 8, 12, and 16 cm. on a side (that is, with volumes of 512, 1,728 and 4,096 cubic centimeters), each weighing 345 grams. Each weight was surmounted by a ring with which it was raised, and by means of which an electric contact was made which indicated when traction began. This with the record when the weight rose represented the time it took to overcome the resistance of the weight without lifting it. He also obtained a record of the height to which the weights were raised. He found that on the average it required 0.12 sec. to overcome the inertia of the largest weight, 0.21 sec. for the medium weight, and 0.62 sec. for the smallest

weight. The largest one was raised 25 mm., the medium one 20 mm., and the smallest one 10 mm. As the illusion causes one to judge the larger weight lighter we have a correlation between the speed with which the inertia is overcome and the judgment of lightness or heaviness, and also a correlation between the height to which they are raised and the judgment; the weight that is judged light being lifted more quickly and to a greater height than the one judged heavy.

These experiments seem to show that before a movement is begun the individual establishes a set to correspond to the anticipated resistance. The anticipation may be of the nature of a habit set up by some such procedure as that of Miss Steffens (11), or it may be by the comparison of the size of the weight to be lifted with the one previously lifted. A change in the speed of the second lift, in the distance to which it is raised, or in the latent time required to overcome the inertia gives the basis for the judgment of lightness or heaviness.

When the compared weights differ greatly it is likely that another factor comes into play, namely a change in the amount of musculature used in the two cases. Révault d'Allonnes (1) obtained graphic records of the struggle which goes on when one tries to lift a weight that is heavier than the lifter anticipates. He arranged an apparatus by means of which a weight is held on a board suspended from a spring so that the slightest change in the force exerted could be graphically recorded. Besides this method he used a false weight which consisted of a jar with no bottom fastened through a table to a very stiff spring, so that it could be raised only with great effort. By the side of this false weight he placed similar appearing weights of differing mass, ranging from 670 to 30,550 grams. The subject was told to estimate the weights in the order in which they were placed, which was from the heaviest to the lightest, and last of all the false one. The graphic records show that in the attempts to raise the false one several trials were made, each one stronger than the preceding. The time of this series of efforts ranged with different subjects from 6 to 18 seconds with an average of 10.05 seconds.

Fullerton and Cattell (3) oppose the Müller-Schumann theory, that time is the basis of judgment of lifted weights, on the ground that their experiments showed that force is more accurately judged than time, and that the perception of force more nearly follows Weber's law than does the perception of time. Besides they found that a person can arbitrarily vary the speed with which the weights are raised and yet judge correctly. Their experiments on the perception of force were complicated with extent so that their first criticism is not unanswerable. They used a spring dynamometer and in all except the heaviest pull the force was a direct function of extent. As to the second objection, the fact that one can arbitrarily change the speed and still judge accurately does not show which is judged more accurately, the force or the time, it only shows that judgment is not confined to time estimations.

Woodworth (13) investigated the relation of extent and force by means of blows. He arranged a lever so that it was operated by a string passing through a pulley to the hand. When the hand descended toward the table it raised the lever and when the hand struck the table the lever continued to move, its extent of movement after the blow being proportional to the force of the blow. He eliminated extent by two methods: one was to place a rod above the table to act as a starting guide to the downward stroke of the hand, the other was to allow the subject to see by the graphic record how far to raise the hand. He however had no control of the time factor and therefore his work shows nothing as to the relation of time and force. He found that gross changes in the force of a blow were accompanied by changes in the extent, but there was no correlation between the smaller changes in force and extent. He gives the following records of the proportion of cases in different experiments in which the finer gradations in force were accompanied by corresponding changes in extent: 62.8 per cent. of 670 cases; 54.6 per cent. of 928 cases; 52.6 per cent. of 207 cases; and 61.7 per cent. of 2,115 cases. The control of the extent by either of the methods that he used did not on the whole increase the accuracy of the force of the blows. He concludes that force is not closely dependent on extent.

We have, in short, one group of experiments which tend to show that our judgment of weights is not based on the amount of force required to lift them, that force is determined by the set of the musculature, but that the judgment is based upon the speed with which they rise or the distance to which they are raised. On the other hand we have experiments that would show that force is an independent entity which may be judged and controlled independently of time or extent.

Experimental

Previous experiments by the writer (9) have shown that, when a subject is instructed to use maximum force throughout an experiment in which various weights are lifted by means of a rope, a change in the mass of the weight to be lifted is not compensated for by the speed of the pull (extent being constant) so as to keep the resultant physical force the same. The results of these experiments showed that the instruction to use maximum force resulted in the subject making the pulls with different weights in nearly the same time, which of course means a marked change in force when the weights are changed.

The first part of the experiment to be described in this paper deals with the *speed* of this adjustment to a change in load, and the second with the *ability* to compensate for changes in load by proper changes in time, so as to keep the resultant physical force the same. The first part was done with the instructions to pull at one's maximum throughout, and the weights changed without the subject's knowledge of the time of the change or of the direction or amount of the change. In the second part the subject was told of the change and allowed to test the weight before making the pulls.

The question which led to the first part of the investigation (that of the speed of adjustment) was whether the adjustment was deliberative, reflex, or a local muscular phenomenon. It was thought that the determination of the speed with which the adjustment was made would throw some light upon this question.

To solve this problem it was necessary to have some mechan-

ism which would permit the subject to raise the weights with great speed and which would give a graphic record of the entire movement. To a carriage which ran in grooves hollowed in horizontal guides was attached a 100 vs. fork in such a position that it would write during the movement of the carriage on a flat smoked surface placed under it. From one end of the carriage a rope was passed through the partition into the next room and attached to the handle which the subject pulled. From the other end of the carriage a rope passed over a pulley on the end of the table on which the guides were fastened, then down and through a movable pulley to which weights could be attached and then up again to a fixed support. By this arrangement the weight moved just one half the distance that the recording carriage moved, and in computing the scores the fact had to be taken into consideration that the acceleration of the weight was one half that of the actual graphic records. As the force equals the product of the mass and the acceleration, what amounts to the same thing is to calculate by the usual formulas using only half the weights in the computation. The weights used, including the weight of the pulley, were 15,540 and 4,880 grams and in the calculations were considered as 7,770 and 2,440 grams.

Eight subjects were used, five of whom knew nothing of the nature of the problem to be studied, while three did. None, however, could know when the weights were to be changed as they were in a separate room, nor could they know the values of the weights. The interval between pulls was the same throughout the experiment, thirty seconds in each case, so that no clue could be obtained from the length of the interval as to the changing of the weights. The subjects were instructed to keep the rope loose between pulls, but as an extra precaution against any index that might be given by the tension of the rope it was knotted where it passed through the wall so that the tension would appear the same. Besides the carriage was locked between pulls till the moment the signal was given to pull, hence the subject could make no tentative pulls before the main one. The signal was given by means of an electric buzzer.

Each subject was given the following directions: "Your task will be to grasp the handle and pull each time you hear the buzzer sound. Pull as far as the rope will permit, exerting all your force throughout the movement. The time taken to respond to the signal will not be measured, so be sure you are perfectly set for the pull before you start it. The force you use is the thing that will be measured, so do your best every single time. Between pulls let the rope hang loose and start each pull from a position which permits the rope to sag."

The first five pulls were allowed as practice, were with the light weight, and were not used in the tabulations. After the first five pulls they were given in the following order: two pulls with the light weight, five with the heavy, three with the light, four with the heavy, three with the light, five with the heavy, four with the light, three with the heavy, five with the light, four with the heavy, and four with the light.

The length of the pull was about 70 cm., the end of which was determined by the weight pulling against a spring fastened to the floor. The fact that the weight moved with only one half the velocity of the arm of the subject, together with the fact that the end of the movement was made by means of a spring, relieved the subject from the blow that otherwise would have been experienced at the end of the movement. Only the first 60 cm. of the movement were recorded, due to the fact that the quick recoil of the spring would have destroyed the last part if the entire movement had been included. The recoil was so rapid that no device we could contrive would stop the vibrations of the fork quickly enough that the return would not blur the true record, and so the smoked surface was removed before the return of the fork. The fact that the movement was 70 cm. in length and the record only 60 cm. made this possible.

In computing the results the graphic records were divided into twelve five-centimeter divisions and the time for each section recorded as accurately as could be done from the markings of a 100 vs. fork. This gave a measure of the speed of each pull in twelve sections. These twelve scores are given in Table I for each subject and the averages for all the subjects

TABLE I

TIME SCORES

Subjects		Sections											
		1	2	3	4	5	6	7	8	9	10	11	12
A	1st L.	3.55	1.93	1.57	1.38	1.26	1.19	1.13	1.07	1.05	1.03	1.02	1.00
	Rem. L.	4.07	2.17	1.75	1.53	1.38	1.27	1.19	1.14	1.10	1.07	1.04	1.02
	1st H.	5.30	2.93	2.40	2.15	1.97	1.84	1.72	1.65	1.59	1.54	1.53	1.52
	Rem. H.	4.83	2.74	2.26	1.98	1.82	1.72	1.63	1.56	1.52	1.48	1.46	1.45
B	1st L.	7.10	3.47	2.62	2.13	1.85	1.59	1.36	1.19	1.07	0.98	0.93	0.89
	Rem. L.	6.24	2.72	2.08	1.72	1.48	1.31	1.18	1.07	0.99	0.93	0.89	0.87
	1st H.	5.20	2.57	2.05	1.77	1.62	1.49	1.39	1.30	1.24	1.19	1.14	1.11
	Rem. H.	5.75	3.08	2.38	2.03	1.79	1.60	1.46	1.34	1.25	1.18	1.12	1.07
C	1st L.	2.89	1.70	1.44	1.30	1.21	1.13	1.07	1.02	0.97	0.93	0.90	0.88
	Rem. L.	3.24	1.86	1.61	1.43	1.30	1.19	1.10	1.04	0.99	0.94	0.90	0.87
	1st H.	3.64	1.88	1.64	1.46	1.34	1.25	1.18	1.12	1.08	1.05	1.02	1.01
	Rem. H.	3.60	2.06	1.65	1.46	1.34	1.25	1.18	1.12	1.07	1.04	1.01	1.00
D	1st L.	7.35	3.16	2.27	1.79	1.50	1.31	1.19	1.11	1.05	1.01	0.97	0.95
	Rem. L.	6.67	2.87	2.07	1.68	1.44	1.28	1.17	1.09	1.03	1.00	0.97	0.95
	1st H.	5.32	2.83	2.19	1.90	1.70	1.58	1.48	1.40	1.34	1.31	1.27	1.25
	Rem. H.	5.85	3.09	2.39	2.03	1.78	1.61	1.48	1.38	1.31	1.25	1.20	1.17
E	1st L.	1.85	1.41	1.14	1.00	0.92	0.87	0.82	0.76	0.71	0.68	0.67	0.64
	Rem. L.	1.95	1.41	1.13	1.00	0.90	0.86	0.82	0.79	0.77	0.74	0.72	0.71
	1st H.	2.95	1.78	1.33	1.26	1.21	1.14	1.09	1.06	1.04	1.03	1.03	1.04
	Rem. H.	2.94	1.65	1.31	1.18	1.10	1.05	1.00	0.96	0.93	0.92	0.91	0.90
F	1st L.	4.28	2.37	1.91	1.66	1.49	1.35	1.22	1.13	1.05	1.00	0.97	0.95
	Rem. L.	4.22	2.19	1.80	1.55	1.39	1.28	1.18	1.10	1.04	0.99	0.95	0.92
	1st H.	5.40	2.59	2.08	1.80	1.60	1.47	1.36	1.28	1.23	1.15	1.10	1.08
	Rem. H.	4.75	2.49	1.98	1.77	1.64	1.53	1.43	1.35	1.27	1.21	1.16	1.12
G	1st L.	2.05	1.42	1.16	1.04	0.96	0.93	0.90	0.88	0.87	0.86	0.84	0.82
	Rem. L.	1.98	1.51	1.22	1.10	1.01	0.97	0.94	0.92	0.90	0.89	0.88	0.87
	1st H.	2.95	1.89	1.59	1.47	1.39	1.34	1.33	1.33	1.34	1.35	1.37	1.40
	Rem. H.	2.92	1.87	1.56	1.47	1.40	1.35	1.32	1.31	1.32	1.33	1.34	1.36
H	1st L.	6.77	2.76	2.06	1.72	1.53	1.39	1.29	1.23	1.18	1.15	1.13	1.12
	Rem. L.	6.37	2.78	2.16	1.82	1.65	1.51	1.41	1.34	1.28	1.24	1.20	1.18
	1st H.	7.70	3.66	2.86	2.46	2.22	2.07	1.93	1.88	1.84	1.78	1.76	1.75
	Rem. H.	7.41	3.42	2.73	2.32	2.08	1.95	1.85	1.76	1.69	1.65	1.64	1.63
Av.	1st L.	4.48	2.28	1.77	1.50	1.34	1.22	1.12	1.05	0.99	0.95	0.93	0.91
P.E.$_m$		*.58*	*.13*	*.13*	*.10*	*.08*	*.06*	*.05*	*.04*	*.03*	*.03*	*.02*	*.03*
Av.	Rem. L.	4.34	2.18	1.73	1.48	1.31	1.21	1.12	1.06	1.01	0.98	0.95	0.93
P.E.$_m$		*.47*	*.13*	*.09*	*.07*	*.06*	*.05*	*.04*	*.03*	*.03*	*.03*	*.03*	*.03*
Av.	1st H.	4.82	2.52	2.02	1.79	1.63	1.53	1.44	1.38	1.34	1.30	1.28	1.27
P.E.$_m$		*.36*	*.14*	*.10*	*.09*	*.08*	*.07*	*.06*	*.06*	*.06*	*.06*	*.06*	*.06*
Av.	Rem. H.	4.76	2.56	2.04	1.78	1.62	1.51	1.42	1.35	1.30	1.26	1.23	1.21
P.E.$_m$		*.36*	*.16*	*.12*	*.09*	*.07*	*.06*	*.06*	*.05*	*.05*	*.05*	*.06*	*.06*

given at the bottom of the table. Each subject is given four scores for each of the twelve sections of the pulls. The first score is the average time in hundredths of a second of the first pulls with the light weight immediately after a change of weight had been made. The second score is the average of the remaining pulls with the light weight. The third score is the average in units of hundredths of a second of the heavy weight immediately after a change from the light one. The fourth score is for the remaining heavy pulls.

In Table II are given the average scores of the last pull

TABLE II

SHOWING THE EFFECT OF THE FIRST CHANGE IN WEIGHTS

Times for Last Light Pull before Change

	Sections											
	1	2	3	4	5	6	7	8	9	10	11	12
Av.....	4.50	2.34	1.87	1.60	1.42	1.27	1.17	1.10	1.05	1.01	0.98	0.96
P.E.$_m$....	.37	.17	.12	.10	.08	.06	.05	.05	.04	.04	.04	.04

Times for First Heavy Pull after Change

	Sections											
	1	2	3	4	5	6	7	8	9	10	11	12
Av.....	5.50	2.68	2.17	1.93	1.77	1.68	1.61	1.56	1.51	1.48	1.46	1.45
P.E.$_m$....	.47	.19	.14	.15	.10	.09	.08	.08	.08	.08	.08	.08

Force for First Heavy Pull after Change

	Sections											
	1	2	3	4	5	6	7	8	9	10	11	12
Av.....	25.8	21.3	18.5	17.2	14.4	13.7	13.0	12.6	11.3	9.7	8.7	
P.E.$_m$....	2.2	1.8	1.0	1.2	.8	.6	1.4	1.1	.9	.8	.9	

with the light weight before any change of weights had taken place, and the first score after the change to the heavy weight was made for the first time. Although the time for the last pull with the light weight before any change in weights occurred is slower than the other average time scores with the light weight it is not significantly so. The first pull with the heavy weight shows a slower time than the other scores with the heavy weight, which is significant throughout the full

extent of the pull (see Fig. 1). It is evident that some form of adjustment took place which did not develop as quickly as the total time of the pull, that is, in not less than 250 sigma. It is possible that this adjustment was one of position. The first heavy weight was a total surprise and as a rule the subject was not sufficiently braced to meet the extra load, consequently he could not bring the extra force to play as readily as in the later pulls when he had a greater or less anticipation of the repetition of such a shock. Several of the subjects stated that after the first pull on the heavy weight they braced themselves more strongly; that is, placed one foot further to the front

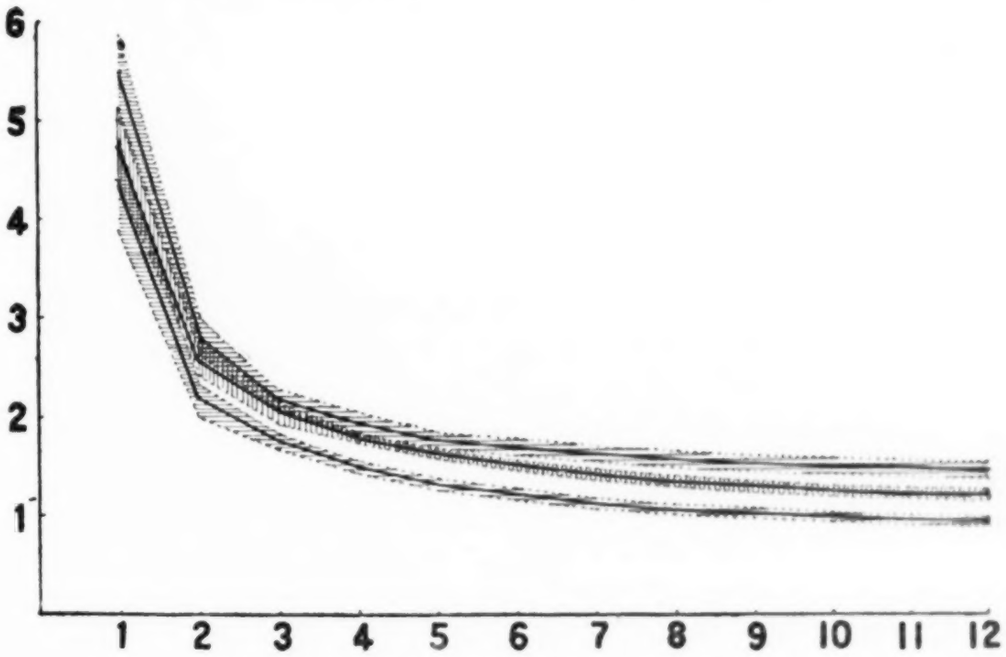

FIG. 1. Graph of the average time scores when the subjects were instructed to use maximum force, with the probable errors at each point represented by the area enclosed within the dotted lines and distinguished by the cross bars. The upper curve represents the first pull with the heavy weight in each experiment, the middle curve represents the pulls with the heavy weight and the bottom curve the pulls with the light weight, the first pull after each change of weights being eliminated.

and took an attitude of greater tension. The average of the first pulls are not reliably different from the average of the remaining pulls, for either the light or heavy weights; showing that after the first shock the changes were met by adequate preparation on the part of the subject.

In Fig. 1 are presented graphically the average time scores, each average being enclosed in a space representing the probable error.[1] The uppermost curve represents the average time taken to pull the heavy weight the first time it was given to each subject. The middle curve is the curve for the heavy weights eliminating the first pull after each change. The lowest is for the light weight with the first pulls after each change eliminated.

In the first part of the pulls the difference in time between the light and heavy weights is not as great as the probable error of the difference. In the fourth section the difference is 3.25 times as great as the probable error of the difference and the reliability increases to the end of the pull when the difference is 4.17 times the probable error of the difference. This shows that in the first part of the pulls the heavy and light weights were lifted with nearly the same speed but that as the pull progressed the heavy weight was pulled more slowly than the light one.

Still further light is thrown on the nature of the adjustments which occurred upon the change of weights by the force scores; but before discussing their significance it may be well to explain how they were derived. For each of the twelve sections of the pull the average velocity was found by dividing the extent by the time; that is, each score in Table I was divided into 5, the extent of each section. The acceleration was then found by subtracting the average velocity of one five-centimeter space from the average velocity of the succeeding five-centimeter space and the remainder divided by the average time for the two spaces. Having found the acceleration the force was next calculated, it being the product of the acceleration plus the acceleration of gravity (980 cm. per sec.) and the mass. In brief the formulas are:

$$v = \frac{s}{t}, \qquad a = \frac{V_2 - V_1}{t}, \qquad F = M(a + 980),$$

in which s = extent, t = time, a = acceleration, V_1 = initial velocity, V_2 = final velocity, F = force in dynes, and M = mass.

[1] The writer is indebted for this method of presentation of the probable error to a suggestion of Professor Cattell.

TABLE III

FORCE SCORES

Subjects	Sections											
	1	2	3	4	5	6	7	8	9	10	11	12
A, 1st L.	12.90	10.60	9.65	8.66	7.36	6.79	5.71	4.69	4.50	4.30	3.84	
Rem. L.	10.75	9.37	8.35	8.42	8.07	7.55	6.15	5.91	5.54	5.39	4.52	
1st H.	22.18	18.38	15.82	15.70	15.37	15.47	13.60	12.89	12.58	9.13	8.62	
Rem. H.	23.72	19.73	18.62	16.38	15.07	15.03	13.44	12.15	11.75	11.31	8.16	
B, 1st L.	5.81	6.07	6.79	6.92	8.35	11.50	12.15	13.15	12.35	9.56	8.84	
Rem. L.	8.05	7.99	9.20	9.26	10.09	10.80	11.48	11.60	10.77	8.84	6.00	
1st H.	28.40	24.39	22.07	19.97	20.58	21.09	21.44	19.25	19.11	18.96	15.93	
Rem. H.	21.00	20.98	20.66	22.30	21.44	22.83	23.70	24.39	22.95	22.45	22.49	
C, 1st L.	15.25	10.80	8.98	8.22	8.24	8.15	8.01	8.03	8.04	7.48	5.68	
Rem. L.	20.20	8.01	8.50	8.81	9.41	9.65	8.54	8.64	8.78	8.76	7.65	
1st H.	43.80	24.80	26.15	24.20	24.39	22.99	23.15	19.65	17.82	17.35	11.45	
Rem. H.	36.20	32.70	27.11	24.20	24.39	22.99	23.15	21.79	18.66	18.22	11.47	
D, 1st L.	6.57	8.86	9.46	10.25	10.90	10.00	8.72	8.50	6.88	6.82	4.92	
Rem. L.	7.46	8.90	9.95	10.05	10.10	9.95	9.07	8.37	6.00	5.86	5.69	
1st H.	23.42	23.07	21.29	21.44	22.10	18.26	18.42	16.12	14.64	13.02	11.92	
Rem. H.	20.82	20.94	20.33	21.09	21.91	20.66	21.21	19.15	18.54	18.39	14.17	
E, 1st L.	15.10	18.25	16.50	13.55	12.20	12.21	16.90	17.65	13.60	11.42	10.21	
Rem. L.	16.75	19.10	15.65	16.55	10.15	9.96	9.65	9.63	8.25	8.08	6.49	
1st H.	43.80	55.00	20.20	18.93	23.50	22.95	16.28	14.26	8.69	7.61	7.56	
Rem. H.	52.70	48.50	34.50	28.00	24.95	24.31	23.46	22.42	13.51	12.70	11.88	
F, 1st L.	9.21	8.32	7.73	7.35	8.74	9.99	9.44	9.79	10.00	6.35	5.18	
Rem. L.	10.75	8.52	8.80	8.38	8.42	8.74	9.02	8.54	8.16	7.70	7.07	
1st H.	27.23	22.91	23.23	23.15	21.80	22.45	21.13	18.14	25.95	20.74	14.00	
Rem. H.	28.20	25.02	20.47	18.10	17.40	18.15	19.89	20.66	20.74	18.77	17.82	
G, 1st L.	17.60	17.30	13.45	12.15	6.77	6.66	6.23	4.89	4.36	6.40	6.50	
Rem. L.	13.55	16.30	11.43	11.88	7.32	6.73	5.54	5.10	4.57	4.32	4.06	
1st H.	37.75	29.90	19.81	19.57	14.44	9.36	7.61	6.44	5.32	4.71	2.64	
Rem. H.	38.40	31.60	17.86	16.81	14.95	12.23	9.38	5.84	5.82	5.80	4.73	
G, 1st L.	7.95	8.69	8.32	7.80	8.08	7.30	6.25	5.83	4.69	3.89	3.25	
Rem. L.	8.38	7.23	7.68	6.46	6.56	6.57	5.58	5.56	5.10	4.98	3.81	
1st H.	17.23	16.89	15.51	14.91	14.13	13.82	10.47	10.12	11.03	10.14	8.50	
Rem. H.	18.38	15.89	16.42	15.66	13.78	13.74	13.20	12.56	11.35	8.55	8.56	
Av. 1st L.	11.29	11.11	10.11	9.36	8.83	9.07	9.18	9.07	8.05	7.03	6.05	
P.E.$_m$	*1.17*	*.99*	*.73*	*.51*	*.41*	*.55*	*.82*	*.93*	*.88*	*.55*	*.55*	
Av. Rem. L.	11.99	10.68	9.95	9.98	8.76	8.74	8.13	7.92	7.15	6.74	5.66	
P.E.$_m$	*1.09*	*1.05*	*.61*	*.64*	*.35*	*.40*	*.53*	*.61*	*.55*	*.48*	*.36*	
Av. 1st H.	30.48	26.92	20.51	19.73	19.54	18.30	16.51	14.61	14.39	12.73	10.08	
P.E.$_m$	*2.54*	*2.32*	*.80*	*.74*	*1.10*	*1.22*	*1.36*	*1.10*	*1.50*	*1.44*	*1.97*	
Av. Rem. H.	29.93	26.92	22.00	20.32	19.24	18.74	18.43	17.37	15.41	14.52	12.41	
P.E.$_m$	*2.81*	*2.40*	*1.32*	*1.07*	*1.18*	*1.19*	*1.44*	*1.61*	*1.44*	*1.48*	*1.29*	

In Table III the force records are arranged to correspond to the time scores in Table I. The first column gives the force scores (in units of 1,000,000 dynes) computed from the times of the first and second sections of the pulls; that is, from columns one and two in Table I. The second column in Table III represents the force scores computed from columns two and three of Table I., etc.

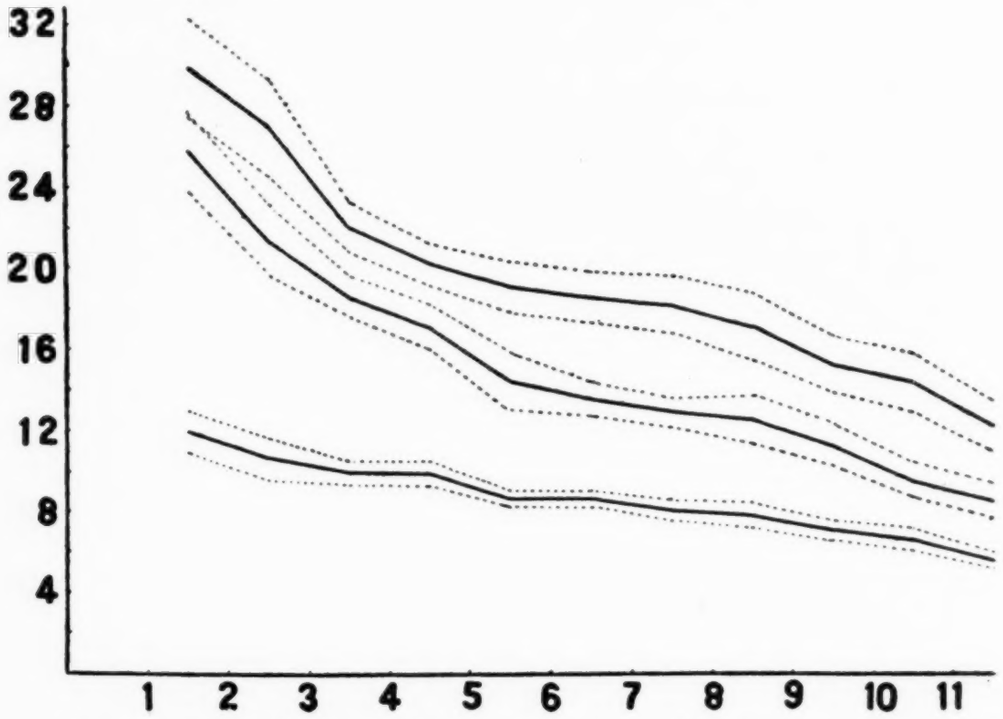

FIG. 2. Graph of the average force scores when the subjects were instructed to use maximum force, with the probable errors at each point represented by the area enclosed within the dotted lines. The middle curve represents the first pull with the heavy weight in each experiment, the upper curve represents the pulls with the heavy weight and the bottom curve the pulls with the light weight, the first pull after each change of weights being eliminated.

The force records (Table III and Fig. 2) show clearly the significance of the fact pointed out in the time scores, namely that the first part of the pulls with either light or heavy weight tend to have the same time values. This of course means that a much greater amount of force had to be used in pulling the heavy weight than was used in pulling the light one. Toward the end of the pulls the difference in force is less marked although it is a highly reliable difference, being five times the probable error of the difference.

The striking fact here is that this large difference in the force used in pulling two greatly differing weights is present at the very beginning of the first pull after the change. That is, after one has been pulling a weight of 2,440 grams with what he supposes to be the maximum force that he is able to exert, when unexpectedly a weight of 7,770 grams is substituted for the lighter one, his force at the very beginning of the pull is on the average 2.5 times as great as the supposedly maximum force previously used.

The time included in the calculation of the force scores in the first column embraces the time it took the subject to pull the weight the first 7.5 cm., or the first time score and half the second. This time ranges from 0.025 sec. to 0.091 sec. with an average of 0.054 sec., much shorter in every case than the simple reaction time, which under the most favorable circumstances can scarcely be reduced to 0.100 sec. (7). In much less time than it takes one to make a simple reaction an adjustment in force can be made when an unexpectedly heavy or light weight is raised. It is evident that this adjustment is either of a reflex nature, or it is something even more elementary.

In every case the greatest amount of force is exerted at the beginning of the pull, the difference between the beginning and end being greater with the heavy weight. Probably this can be explained by an analysis of the manner of pulling in the light of the data already presented. Each pull was started from a position which allowed the rope to sag, the subject setting himself for his maximum pull, which doubtless was to pull as quickly as possible; hence the speed at the initial part of the pull was about the same in all cases. After the first impulse the light weight continued at a rapid rate with little continued force while the heavy one would require more force to keep it going at the pace given by the first impulse. With jerks at the beginning which would start both light and heavy weights at the same rate the light weight would end the movement at a greater rate of speed than the heavy one.

While we have shown that one's execution of what he regards as his maximum force is determined by the amount

of resistance opposed to the movement involved and while the quickness with which the force adjustment is made indicates that it is of a reflex or some simpler nature, it is not so certain that under more favorable circumstances one could not make a proper time adjustment to a change in weight and keep the resultant physical force the same. In order to see with what degree of accuracy such an adjustment could be made, another experiment was performed in which the subjects were told just what would take place.

The directions given were as follows: "The object of this experiment is to test your ability to use the same force in pulling different weights. There will be four different weights and you will be given the next to the heaviest first. You will always be notified when a weight is changed and given the privilege of feeling how heavy it is before making the pulls. Three pulls will be made between changes and you will be asked to judge in which of the three you think you used the nearest to the same amount of force as with the preceding weight. Make a pull each time you are given the buzzer signal to do so, and start each pull from a position which will allow the rope to sag. *Remember* it is the force you use that you are to keep the same regardless of the time it takes you to make the pull."

Four weights were used instead of only two as in the first experiment of the paper. These weights were 15,540; 12,180; 9,300; and 4,880 grams; which, due to the fact already stated that they moved only one half the distance of the recording carriage, were treated in the tabulations as weights of 7,770; 6,090; 4,650; and 2,440 grams. Each weight was given at three different times during the experimental period and at each presentation was given three pulls. After the first three pulls the subject was asked which one of the three he judged nearest to what he wished to keep as a standard. Thereafter after each three pulls he was asked to judge which of the three came nearest to the standard he had so chosen. No tabulation of the accuracy of these judgments is presented since the poorest record was as often chosen as the best, and all the subjects said that they were merely guessing and had not the least idea how nearly they were coming to the same force.

If we let I. represent the 2,440 gram weight, II. the 4,650, III. the 6,090, and IV. the 7,770, the order in which they were given was III., I., II., IV., I., II., III., IV., III., IV., I., and II. In the calculations only the last eight presentations were used. The first four were regarded as practice pulls, for although the subjects were told that next to the heaviest was to be given first they could not adjust themselves to this information. Two of the subjects when they came to the heaviest weight for the first time said that if they used the same force that they had been using they could not pull it at all. They were then told to pull IV. to suit themselves and to try to use this new standard throughout the remainder of the experiment.

It may seem from this that the subjects had some idea of what it meant to use the same force. When asked what they meant by changing the amount of force in order to pull the heaviest weight, they stated that they had to change their posture, that they had been controlling their former pulls by setting their feet a certain distance apart and pulling so that with the same set of their musculature they were neither drawn forward nor moved backwards when they pulled. When the heavy one came they felt that this posture was not sufficiently stable and experienced a pull forward, hence the necessity for readjustment of position. We may state here that the weights were of such a range that it was physiologically possible to adjust the time of pulling so that each could be pulled with the same force.

Eight subjects were used, two of whom had served in the first experiment. In this experiment two of the subjects were women, *J* and *L*. On reading the directions most of them seemed bewildered and their remarks and attitude throughout the experiment showed that the task of keeping force the same with different weights was one to which they were unaccustomed. Most of them reassured themselves before they began by asking if the directions did not mean that they must pull the heavy weights slower than the light ones. The fact that they required such information is an indication that they had a very crude idea of force as such. They simply

TABLE IV

TIME SCORES

Subjects	Wt.	Sections											
		1	2	3	4	5	6	7	8	9	10	11	12
I.....	I.	8.2	3.4	2.7	2.3	2.0	1.9	1.9	1.9	1.9	2.0	2.1	2.2
	II.	7.6	3.9	2.9	2.6	2.3	2.1	2.2	2.2	2.2	2.4	2.7	2.8
	III.	10.8	5.3	3.9	3.4	3.2	2.9	3.0	3.0	3.0	3.2	3.6	4.2
	IV.	13.8	8.6	7.4	6.8	6.4	6.1	5.9	5.6	5.4	5.7	6.0	7.1
B.....	I.	8.7	4.1	3.2	2.7	2.4	2.2	2.1	2.0	1.9	1.9	1.87	1.83
	II.	8.8	4.7	3.5	3.0	2.7	2.5	2.5	2.4	2.4	2.4	2.4	2.6
	III.	9.5	5.1	3.8	3.3	3.1	2.8	2.9	2.9	2.9	3.0	3.1	3.4
	IV.	10.3	5.4	4.2	3.6	3.4	3.1	3.2	3.2	3.2	3.4	3.5	3.9
J.....	I.	15.3	7.3	5.8	4.8	4.4	4.0	4.0	4.0	4.3	5.1	6.8	9.8
	II.	30.3	12.3	9.6	8.4	8.0	7.8	7.7	8.1	8.7	9.8	10.9	12.1
	III.	27.5	14.6	12.6	13.0	12.1	12.4	11.8	10.7	11.6	12.0	13.6	16.8
	IV.	31.0	17.0	13.3	13.9	14.2	14.3	15.3	13.9	14.3	15.8	14.3	17.0
G.....	I.	3.37	2.21	1.87	1.75	1.62	1.58	1.58	1.54	1.54	1.58	1.62	1.67
	II.	3.75	2.37	2.08	1.87	1.83	1.75	1.75	1.79	1.87	1.96	2.04	2.17
	III.	4.75	2.75	2.46	2.21	2.08	2.04	2.00	2.00	2.00	2.10	2.20	2.40
	IV.	4.62	2.96	2.62	2.37	2.25	2.21	2.17	2.25	2.37	2.58	3.00	3.00
K.....	I.	6.5	3.08	2.46	2.12	1.92	1.79	1.58	1.5	1.49	1.42	1.42	1.46
	II.	10.3	5.6	4.4	3.9	3.8	3.8	3.7	3.7	3.8	3.8	3.8	4.0
	III.	10.3	6.5	5.6	5.0	5.0	5.1	5.4	5.5	5.3	5.0	5.1	5.4
	IV.	13.5	9.3	8.6	7.7	7.8	8.3	12.0	8.5	7.8	8.0	8.8	12.3
L.....	I.	4.2	2.6	2.0	1.8	1.6	1.5	1.4	1.3	1.27	1.24	1.22	1.2
	II.	6.3	3.6	2.9	2.5	2.2	2.0	1.9	1.8	1.7	1.7	1.75	1.79
	III.	6.5	3.8	3.2	2.7	2.4	2.3	2.25	2.2	2.1	2.1	2.12	2.17
	IV.	8.0	4.7	3.8	3.5	3.1	3.0	2.8	2.8	2.8	2.8	3.0	3.2
M.....	I.	3.8	2.3	2.0	1.8	1.6	1.5	1.5	1.5	1.5	1.5	1.54	1.58
	II.	4.5	2.8	2.3	2.0	1.8	1.7	1.7	1.6	1.6	1.6	1.65	1.7
	III.	5.6	3.4	2.7	2.4	2.2	2.1	2.0	2.0	2.0	2.1	2.2	2.2
	IV.	6.0	3.6	3.0	2.5	2.3	2.2	2.1	2.05	2.0	2.04	2.09	2.15
N.....	I.	14.2	16.0	16.3	15.2	14.4	14.1	11.6	11.4	10.9	9.5	8.5	8.2
	II.	26.3	15.4	14.3	14.3	13.1	13.0	11.9	12.0	13.2	13.0	11.4	9.4
	III.	31.7	18.1	15.2	15.7	16.5	14.8	14.7	13.5	14.1	14.0	13.8	13.5
	IV.	25.7	16.1	13.1	14.0	14.3	13.7	12.2	12.6	13.2	11.5	9.9	10.6
Av.....	I.	8.0	5.13	4.53	4.06	3.74	3.70	3.21	3.14	3.10	3.03	3.13	3.50
P.E.$_m$...		*1.1*	*.9*	*.9*	*.9*	*.8*	*.8*	*.8*	*.8*	*.7*	*.6*	*.8*	*.9*
Av.....	II.	12.2	6.27	5.2	4.82	4.47	4.32	4.17	4.20	4.42	4.56	4.57	4.57
P.E.$_m$...		*2.4*	*1.0*	*1.0*	*1.0*	*.9*	*1.0*	*.8*	*.9*	*1.0*	*1.0*	*1.0*	*1.0*
Av.....	III.	13.3	7.42	6.17	5.95	5.7	5.55	5.55	5.23	5.37	5.42	5.71	6.26
P.E.$_m$...		*2.4*	*1.1*	*1.2*	*1.1*	*1.3*	*1.2*	*1.2*	*1.1*	*1.1*	*1.1*	*1.1*	*1.1*
Av.....	IV.	14.1	8.45	7.00	6.80	6.7	6.61	6.95	6.37	6.38	6.47	6.32	7.42
P.E.$_m$...		*2.1*	*1.3*	*1.5*	*1.1*	*1.2*	*1.3*	*1.4*	*1.2*	*1.2*	*1.2*	*1.2*	*1.3*

tried from what they knew of the laws of dynamics to compensate for a heavy weight by a slower speed.

In this case the scores were calculated as before, that is the total extent of the record, 60 cm., was divided into twelve sections and the time taken for each section. In Table IV four lines of scores are given for each subject; I. the times in hundredths of a second for each section with the 2,440 weight, II. for 4,650, III. for 6,090, and IV. for 7,770. From the time scores the force scores in Table V were derived by means of the formulas given above. The force unit is again 1,000,000 dynes. The averages of these two tables are graphed in Figs. 3 and 4, with the areas representing the range of their probable errors embraced by dotted lines.

The first thing evident is that the time scores do not coincide as nearly as they did when the instructions were to use maximum force throughout. This is no doubt the result of the deliberate attempt to change the time to compensate for changes in the weight. Taken by themselves the time scores might seem to indicate the ability to make such an adjustment, but a glance at the force records will show that the adjustment is at best very poor. The difference in the first time scores between the light and heavy weights is 2.5 times the probable error of the difference, the difference in the last time scores 1.6 times the probable error of the difference. The difference between the lightest and heaviest is for the first force score 6 times the probable error of the difference, for the last force score 6.05 times the probable error of the difference. The relative reliability of the time and force records can be seen clearly by the relative overlapping of the probable error areas in Figs. 3 and 4.

In this experiment the pulls were of a different nature from those when the subject tried to use maximum force; they were less violent and the force was sustained more throughout the extent of the movement. Even here however more force was used at the beginning than at the end of the pull. The time curves show a rapid increase in rate at the beginning and then a fairly even rate through the remainder of the pull with a slight indication of a diminution of speed at the end.

TABLE V

FORCE SCORES

Subjects	Wts.	Sections 1	2	3	4	5	6	7	8	9	10	11	12
I.....	I.	6.00	4.99	5.55	6.07	4.01	2.39	2.39	2.39	0.76	0.84	1.25
	II.	9.23	10.62	7.90	9.08	8.82	2.18	4.56	4.56	0.81	0.29	2.33
	III.	9.00	10.45	9.11	7.64	9.17	4.79	5.97	5.97	3.87	2.90	2.86
	IV.	9.13	8.52	8.28	8.14	8.12	8.96	8.24	8.08	6.93	7.04	5.94
B.....	I.	4.84	4.65	5.76	4.62	4.35	3.63	3.82	4.01	2.39	2.90	3.17
	II.	7.83	8.70	7.97	7.55	7.22	4.56	5.68	4.56	4.56	4.56	1.55
	III.	9.62	10.53	9.44	7.83	9.53	4.70	5.97	5.97	4.79	4.90	3.29
	IV.	11.95	11.88	11.54	9.41	11.04	6.31	7.61	7.61	5.49	6.48	4.52
J.....	I.	3.16	3.15	3.22	2.94	3.05	2.39	2.39	1.89	1.43	1.39	1.73
	II.	5.08	5.04	4.93	4.71	4.65	4.60	4.34	4.32	4.21	4.32	4.39
	III.	6.43	6.22	5.92	6.11	5.92	6.07	6.22	5.77	5.90	5.73	5.70
	IV.	8.04	8.04	7.51	7.57	7.58	7.49	7.78	7.55	7.43	7.78	7.34
G.....	I.	8.15	7.28	4.82	4.87	3.54	2.39	3.72	2.39	1.07	1.24	1.06
	II.	16.41	10.62	10.90	6.07	7.92	4.56	2.72	1.51	1.52	2.35	1.13
	III.	18.38	10.98	11.96	9.93	7.45	7.48	5.97	5.97	2.41	2.86	0.94
	IV.	19.65	14.74	12.66	10.64	10.92	7.61	4.34	4.63	2.56	0.58	0.19
K.....	I.	6.72	6.00	5.86	5.34	4.87	5.01	5.14	2.73	5.25	2.39	0.69
	II.	6.94	6.81	6.20	4.94	4.56	5.02	4.56	4.09	4.56	4.56	3.81
	III.	8.03	7.21	7.20	5.97	5.73	5.34	5.77	6.37	6.64	5.73	5.34
	IV.	8.00	8.00	8.26	7.54	7.23	6.20	8.90	8.12	7.44	7.09	6.41
L.....	I.	7.65	8.54	5.93	7.42	5.66	6.29	7.30	3.85	4.32	3.79	3.59
	II.	10.13	9.35	9.29	9.87	9.63	7.66	8.21	8.95	4.56	2.40	2.67
	III.	12.39	10.25	11.96	11.56	8.31	7.17	7.35	9.08	5.97	5.39	3.42
	IV.	12.97	12.23	10.06	11.95	8.98	10.78	7.61	7.61	7.61	4.46	4.92
M.....	I.	5.83	6.07	5.92	7.42	5.66	2.39	2.39	2.39	2.39	0.93	1.29
	II.	12.14	11.67	11.57	11.30	8.93	4.56	9.76	4.56	4.56	0.97	1.09
	III.	13.76	13.58	11.53	10.86	9.09	9.53	5.97	5.97	2.40	2.86	5.97
	IV.	16.58	13.97	17.00	13.28	10.90	11.56	9.48	10.29	5.78	5.36	4.81
N.....	I.	2.33	2.38	2.42	2.41	2.40	2.54	2.40	2.43	2.55	2.53	2.45
	II.	4.83	4.63	4.56	4.67	4.57	4.68	4.55	4.41	4.58	4.74	4.97
	III.	6.25	6.17	5.94	5.91	6.11	5.97	6.09	6.03	5.97	5.99	6.00
	IV.	8.04	7.98	7.45	7.57	7.68	7.87	7.53	7.72	7.96	7.96	7.86
Av.....	I.	5.58	5.38	4.81	5.14	4.19	3.38	3.69	2.76	2.52	2.00	1.90
P.E.$_m$....		*.47*	*.47*	*.46*	*.43*	*.28*	*.36*	*.39*	*.18*	*.34*	*.27*	*.26*
Av.....	II.	9.07	8.43	7.91	7.27	7.04	4.73	5.55	4.62	3.67	3.02	2.74
P.E.$_m$....		*.79*	*.66*	*.61*	*.65*	*.55*	*.20*	*.52*	*.32*	*.30*	*.46*	*.37*
Av.....	III.	10.48	9.42	8.12	8.23	7.66	6.38	6.16	6.39	4.74	4.54	4.19
P.E.$_m$....		*.83*	*.65*	*.40*	*.57*	*.41*	*.38*	*.09*	*.20*	*.41*	*.38*	*.46*
Av.....	IV.	11.79	10.67	10.34	9.51	9.05	8.35	7.68	7.70	6.40	5.59	5.25
P.E.$_m$....		*.93*	*.76*	*.75*	*.55*	*.43*	*.40*	*.28*	*.26*	*.40*	*.63*	*.49*

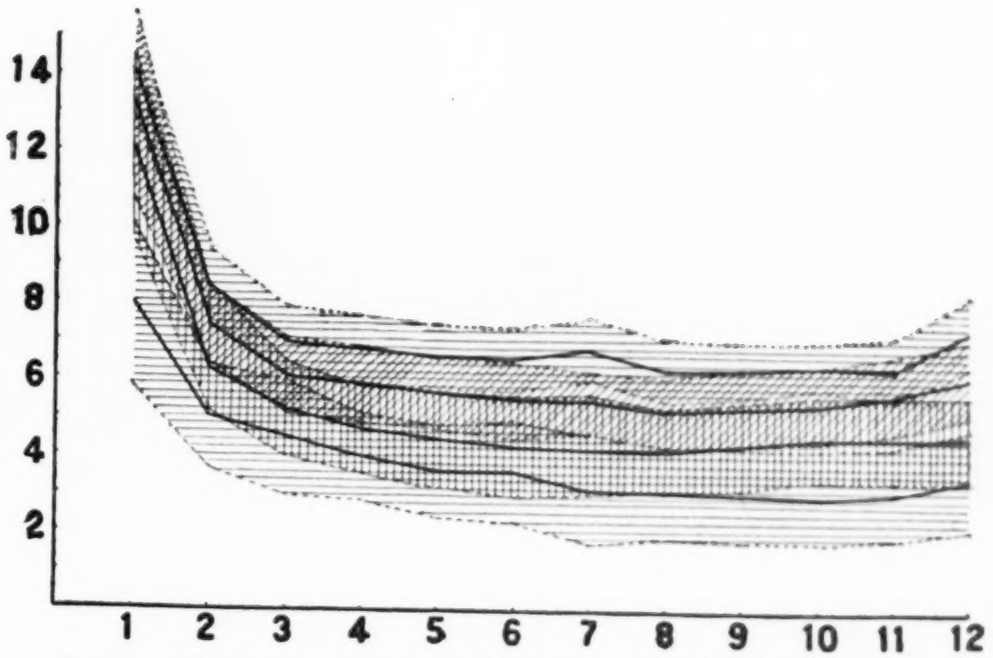

FIG. 3. Graph of the average time scores in the experiment where the subjects tried to work with equal force with different weights. The probable errors at each point are represented by the area enclosed within the dotted lines and distinguished by the cross bars. From top to bottom the curves represent the times with weights of 7,770; 6,090; 4,650, and 2,440 grams.

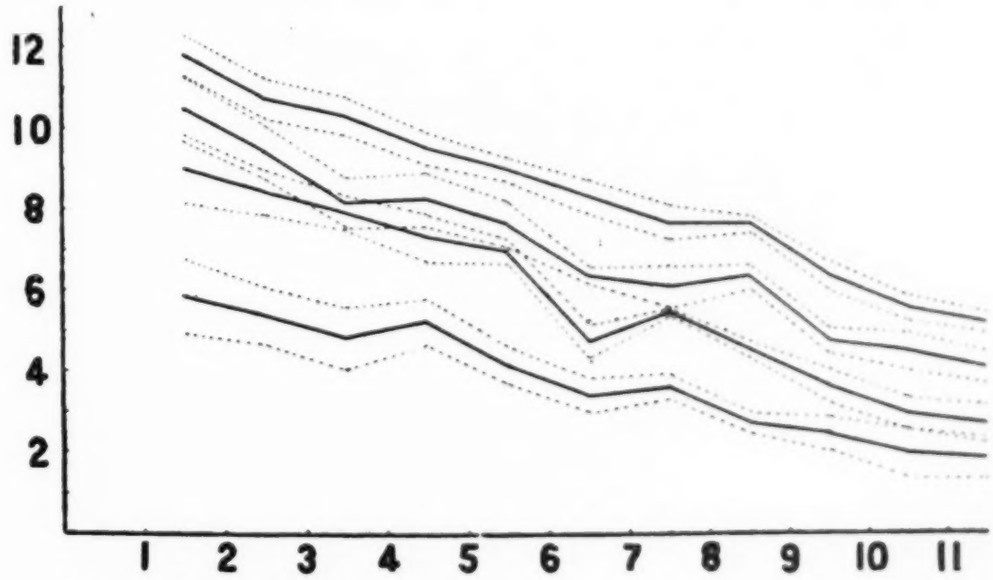

FIG. 4. Graph of the average force scores in the experiments where the subjects tried to work with equal force with different weights. The probable errors at each point are represented by the area enclosed within the dotted lines. From top to bottom the curves represent the force used with weights of 7,770; 6,090; 4,650, and 2,440 grams.

This is evidence that one has no idea how to accelerate a movement so that the force would be constant throughout. Constant force throughout a movement would require a uniform increase in rate for each unit of space. In such a case the time curve would be a straight 45° line in which for each unit of extent there is an equal increase in speed. Instead of that the subjects tended to maintain uniform velocity. In the first part of the movement they quickly caught the velocity they judged proper for that weight and then maintained that velocity at least approximately throughout the pull. They certainly had no idea of the control of force in the sense in which it is regarded in physics. They regarded uniform speed throughout a pull as uniform force.

Conclusions

None of the work done on the judgment or control of force has eliminated both the factors of extent and time or varied the conditions so that the control of the force of a movement as an independent entity could be determined. It is possible that in some of the experiments the controlling factor was time and in others extent, while in some both factors could feature as far as the controls used in the experimental procedure were concerned. We therefore do not regard the data we have obtained as contradictory to that derived from previous experimentation, we simply believe that the work done has not been of the nature to answer the question as to whether one can control force independently of extent of time. Even our experiments do not show conclusively that one cannot control force at all. What it does show is that one has not the ability to adjust the speed of a movement so that with different loads the movement can be made with the same physical force. The *implication* from this would be that one cannot control the amount of force he is using, and that what control he does use is based on some other factor. To test the validity of this implication the writer is working on a different series of experiments which he hopes will show the relative accuracy of the control of force, extent, and time.

The speed with which force is adjusted to the size of the load shows that it is a very elementary process. The question is still left whether it is a local muscular phenomenon or a reflex. If it is a reflex it is a very rapid one, for in some cases the adjustment took place in less than 0.025 sec. It is very likely that it may be a local phenomenon of the muscle itself. Physiologists have found a similar phenomenon in the nerve muscle preparation of the frog. Luciani (6) says: "According to the observations originally made by Fick, and afterwards confirmed by others, when the weight applied to the muscle is not great, and particularly when an elastic resistance is opposed to the muscle, so that its tension increases constantly during contraction, the shortening is greater when the weight and the initial resistance are increased. This paradoxical phenomenon is a specific property of the substance of living muscle, and shows that the sudden pull of the muscle and increase of tension during shortening act as a stimulus on the contractile substance, and increase the effect of the electrical stimulation."

While the quickness of the adjustment in the procedure where maximal force was required may be explained as a local muscular phenomenon it does not show that this is the case where the subjects chose the speed of their pulls and were aware of the size of the weight before they pulled. It is not hard to see, however, how it could be of such a nature even in such a case. If it was controlled by anything above a reflex center the adjustment could certainly be made with greater accuracy than our records show. In short, when the extent is kept constant and the load changed one judges his force by the time of his pull; when he is told that the weights are changed, knowing that his pull should be slower with the heavier, he makes a time correction but it is a very, very crude one.

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THE JOHNS HOPKINS CHRONOSCOPE

BY KNIGHT DUNLAP

The chronoscope illustrated herewith is the second model, of which nine instruments have been made. The first chronoscope, which has been in operation for over a year, differs from these later ones in the details of the clutch mechanism, but operates in essentially the same way. Twenty-five instruments of the third model, which has some improvements in details of construction, are now being constructed.

The chronoscope is fundamentally a 10-pole synchronous motor (Fig. 1) constructed with extreme care. The shaft bearings are adjustable and replaceable. The motor will run on direct current, interrupted by a tuning fork, from 15 to 100 times per second, or will run without interruption on an alternating current at frequencies of from below 10 to above 90.

The rotation rate of the armature when run on direct current is one tenth the number of interruptions per second, *e. g.*, with a 50 vibration fork, the armature makes five rotations per second. On the alternating current the rotations are one fifth of the frequencies per second, *e. g.*, on the 60-cycle current, the armature makes twelve rotations per second.

The motor operates continuously with very little noise. If it is desirable, the electric fork may be put in another room or may be enclosed in a padded box with a thermostat to keep the temperature uniform. Temperature changes have no effect on the chronoscope mechanism itself: the only part of the system which can be influenced by temperature changes is the tuning fork.

The clutch mechanism, through which the hand is alternately engaged with the motor shaft and braked to a standstill, consists of two simple electromagnets (K' and K''), the core of one (K'') being fastened to the armature shaft and rotating with it. The core of the other (K') may be

rotated by turning the milled ring (*F*), but is normally at rest. The windings of both magnets are fixed in position so that there is no traveling or moving electric contact. When the current is off both magnets the hand, *J*, on the dial plate may be set back to zero by turning the milled head *H*, attached to the large gear wheel *G* (Fig. 2). When the current is on the forward magnet (*K'*, Fig. 1) the hand may be moved by turning the milled ring, *F*. When the current is on both

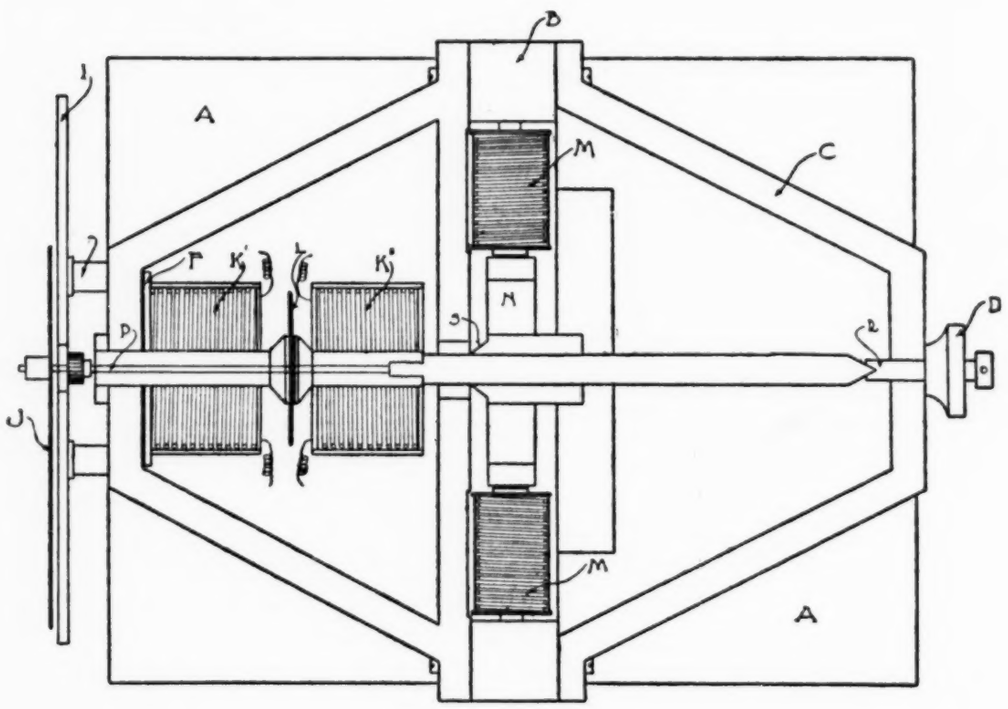

FIG. 1.

magnets, the armature disc, *L*, carried by the hand shaft, *P*, will stay against either magnet core. It may be, therefore, moved over from one to the other by breaking first the one circuit and then the other, just as is the case with my method of using the Hipp chronoscope without armature springs. In this way the effect of variations in the electric current actuating the clutch mechanism are entirely avoided. There is a trifle of slip in starting and stopping with this new mechanism, but the error amounts to less than one *sigma* on the average, which is a negligible magnitude for all reaction time work.

The dial, *I* (Fig. 3), is five inches in diameter, and has in the upper portion an aperture, *T*, through which the large gear wheel, *G* (Fig. 2), is visible. This wheel carries numerals, legible through the aperture, which indicate complete rotations of the index hand.

The advantages of the Johns Hopkins Chronoscope are: first, that it may be operated with practically no noise; second, that it requires no winding, since it will run for several hours at a time without further adjustment of the fork contacts; third, that the dial is large and easily read; and fourth, that the hand is readily set back to zero, avoiding the necessity of subtraction.

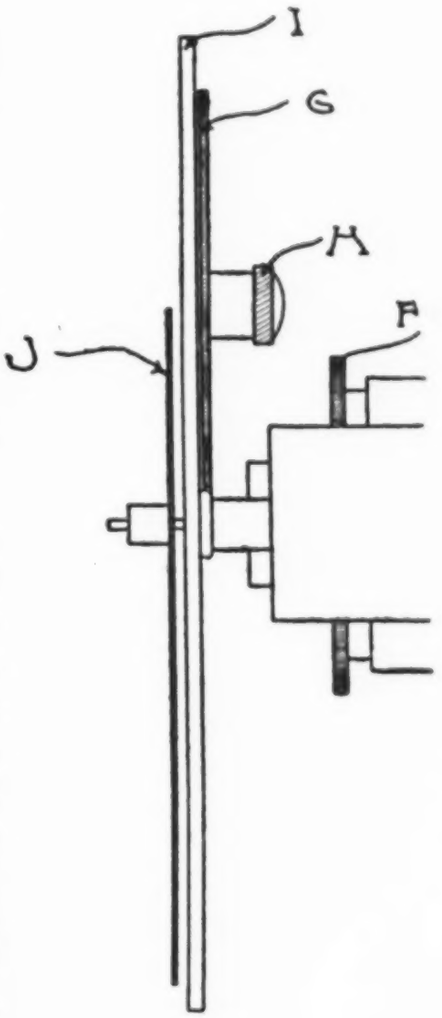

FIG. 2.

The synchronous motor was invented independently by Lord Rayleigh (who published an account of his "phonic wheel" in 1879), and by La Cour (somewhat earlier than Rayleigh). Numerous applications to scientific and commercial purposes have since been made, among them the use in a printing telegraph system, patented by Professor Rowland of the Johns Hopkins University in 1896. My first attempts at construction of a synchronous motor for driving a time machine (in 1908–10) were not completely successful. In 1914 Dr. Lorenz of the Nela Research Laboratory kindly loaned me a large synchronous motor which he had reconstructed from an ordinary A.C. motor, and after using this and discussing the matter with Dr. Lorenz I designed the motor which is the basis of the Johns Hopkins chronoscope. This motor embraces Lorenz's plan of a multipolar field (thus departing

from the Rayleigh, Rowland and Bull models) but in the details is original. My indebtedness to Dr. Lorenz (who also

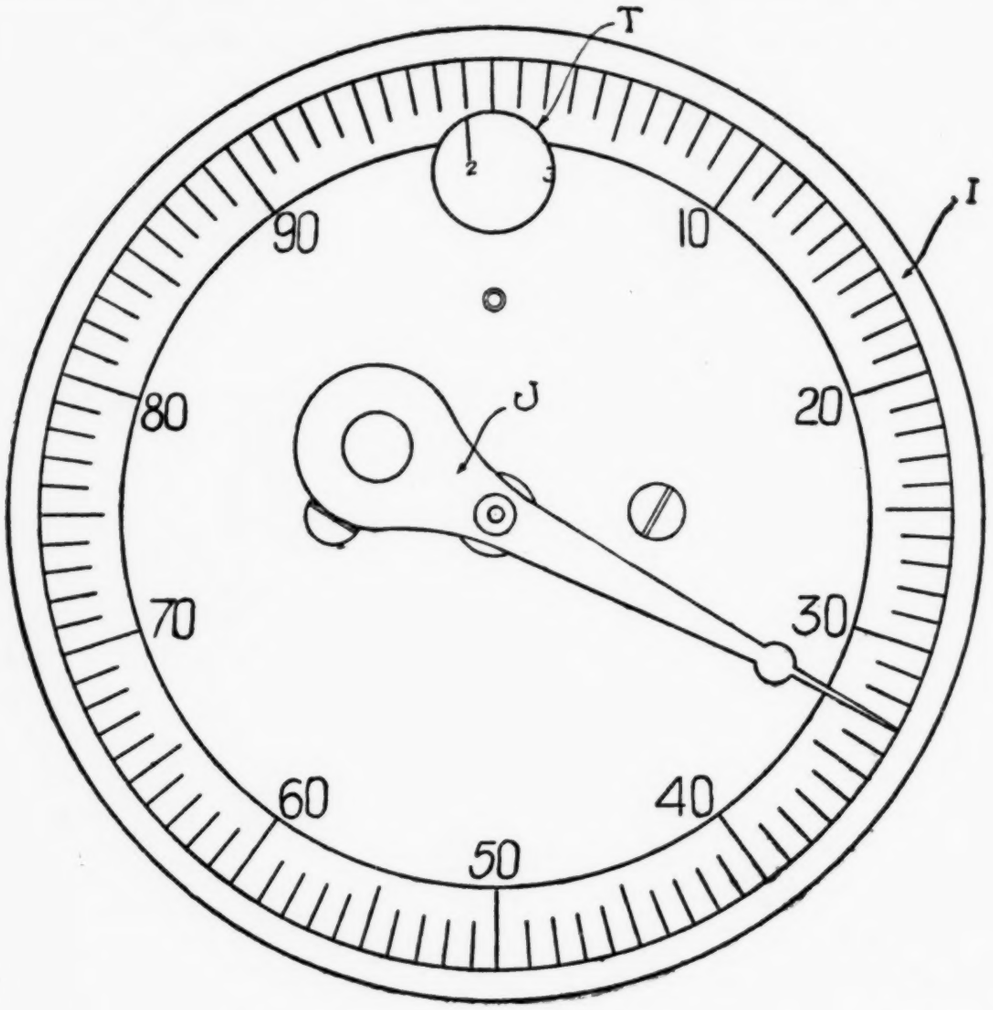

FIG. 3.

applied the synchronous motor to the Seashore tonoscope) is gratefully acknowledged.